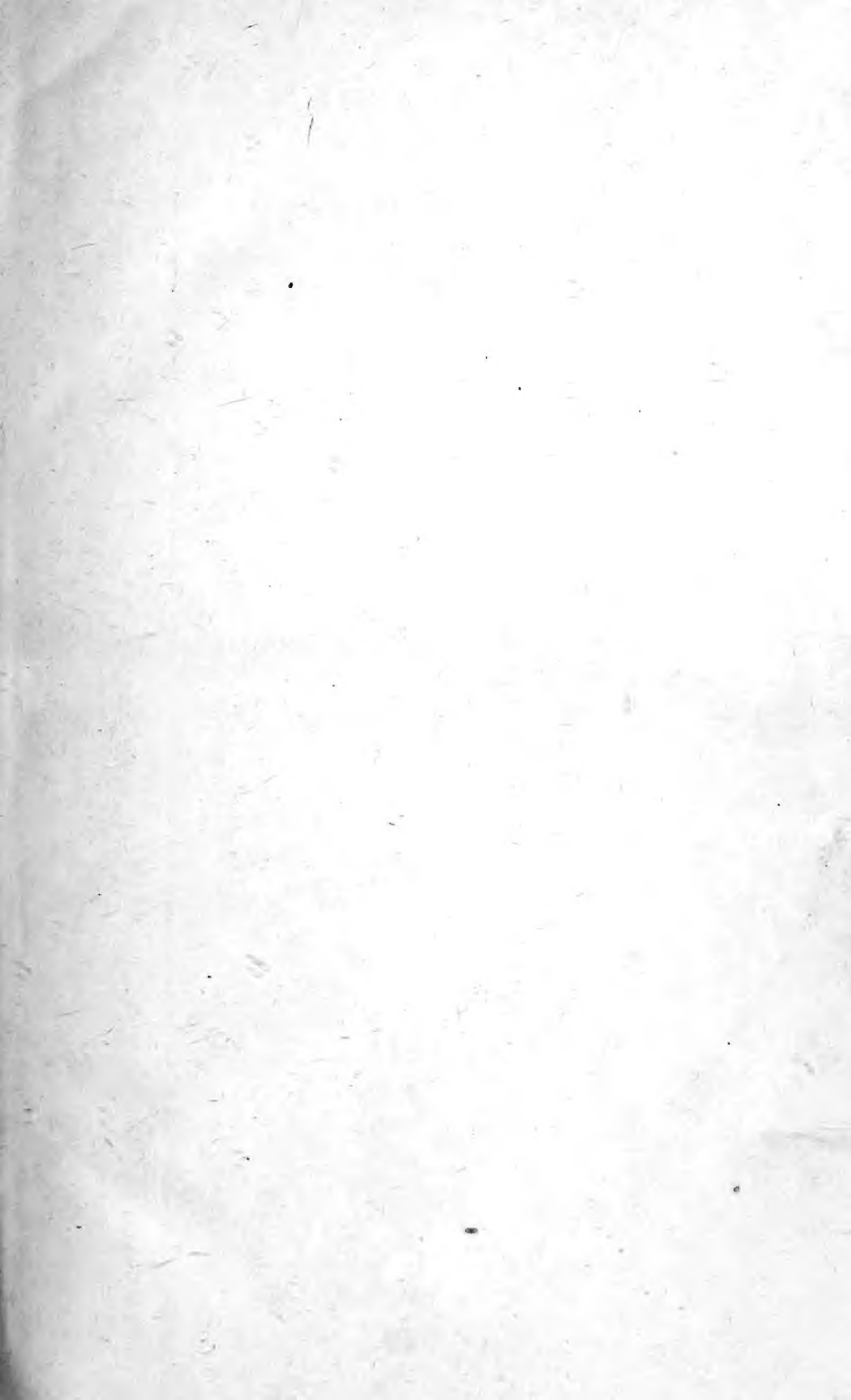L. R. 1.

ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS,

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

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VOL. IV.

JULY TO DECEMBER, 1814.

NOTICE.

AT length this country is regularly supplied with foreign Journals; so that it is in the power of the Editor of the *Annals of Philosophy* to lay before the British Public the scientific improvements as they are made in the different countries of Europe. Already several important papers from the foreign Journals have made their appearance in the *Annals of Philosophy*. The great quantity of original matter, indeed, with which the pages of the *Annals* are filled, preclude the possibility of going beyond a certain length in such selections; but the Editor flatters himself that he has fallen upon a way which will enable him to make his British readers acquainted with what has been done on the Continent, in science, during the last eight years, without taking up any extraordinary portion of the *Annals* in the detail. This attempt will be seen at the commencement of the next volume.

It is a very singular fact, which we do not pretend to explain, though the French booksellers in London probably can, that the supply of Paris Journals is more irregular and dilatory than of any other, though the distance of that capital from our own is so small. It would be a very desirable thing for the Editors of scientific Journals, and for the Public in general, if this defect, the result of inattention in some quarter or other, were remedied.

November, 1814.

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ANNALS

OF

PHILOSOPHY.

JULY, 1814.

ARTICLE I.

Remarks on the Biographical Account of M. Lagrange by M. Delambre, with some Additions. By L. B. M. D. G.*

TOWARDS the commencement of the account of M. Lagrange, Halley is named as the author of a paper written on purpose to demonstrate the superiority of the modern analysis, and which induced Lagrange to abandon the exclusive study of ancient geometry. But, admitting the fact, is it likely that the paper in question was really written by Halley.† Not having it in my power to consult the Philosophical Transactions, I cannot venture to contradict the statement. Yet I have my doubts respecting its truth; because Halley devoted a considerable part of his life in restoring the works of Apollonius, one of the most famous of the ancient mathematicians; because in his numerous writings he constantly panegyrized them, and exhorted geometricians to study them; and because he entertained the same prejudice with his friend Sir Isaac Newton in favour of pure geometry, and contributed very much to

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* Translated from the Moniteur of Feb. 26, 1814. Some of the additional facts which this paper contains are too valuable to be withheld from the British public.—T.

† The anecdote respecting Halley was related by M. Lagrange at a meeting of the Board of Longitude. The memoir in question is that in which Dr. Halley gives the resolution of the problem of finding the foci of optic glasses. Halley was an admirer of ancient geometry, but did not dispute the superiority of modern analysis. Thus with respect to logarithms he deduced from the binomial theorem, and the calculus of fluxions, expressions much preferable to the laborious methods of the first inventors.—*Note by the Editor of the Moniteur.*

I have had the curiosity to look at Dr. Halley's memoir. It is in Phil. Trans. vol. xvii. p. 960; and entitled, " An Instance of the Excellence of Modern Algebra, in the resolution of the problem of finding the foci of optic glasses universally." It begins with the panegyric in question.—T.

induce Newton to give that synthetic form to the Principia which they possess; and because, if we except a small number of points respecting algebraic calculus, and the theory of equations, he has treated in his writings of none of the points which constitute modern analysis.

It is stated likewise that Lagrange was Professor of the Artillery Academy at the age of sixteen.* But if rigid accuracy be required, this statement seems to me doubtful; for Lagrange has often told me that he did not begin the study of mathematics till 1753, when he was seventeen years of age; and he told me (only once indeed) that he was named Professor at the age of nineteen.

As it may be an object of curiosity to know his first labours, I shall state them according to his own information. He studied first arithmetic, the elements of Euclid, and the algebra of Clairaut: then in less than two years he read the following books, in the order in which I name them: the Analytical Institutions of Mademoiselle Agnesi, the Introduction of Euler, the Lectures of John Bernoulli, the Mechanics of Euler, the two first books of the Principia of Newton, the Dynamics of D'Alembert, the Integral Calculus of Bougainville, and the Differential Calculus and Methodus Inveniendi of Euler. It is well known that it was this last work which led him to the discovery of the *calculus of variations*.

While speaking of the origin of this brilliant discovery, such as Lagrange himself related it two days before his death, the author has inadvertently allowed an error of some importance to escape him, which renders the details of this subject very obscure.† The discovery of the method of variations is confounded with one of its finest applications, the general theorem of mechanics, to which the name of *the principle of the least action* has been given. To render this evident it will be necessary to quote the following passage from the biographical account of Lagrange.

The first attempts to determine the maximum and minimum in all indefinite integral formulas were made upon the occasion of the curve of swiftest descent, and the isoperimetres of Bernoulli. Euler had brought them to a general method, in an original work in which the profoundest knowledge of the calculus is conspicuous. But however ingenious his method was, it had not all the simplicity which one

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* The statement, sixteen years, was made in consequence of the last conversation of Lagrange with M. Chaptal. The Journal de l'Empire, of the 28th April, 1813, gave the same age. M. Virey in his notice, printed about the same time, said fifteen years; but M. Cossali, in his Eloge printed in Italy, says nineteen years; that is to say, in 1755. By that time Lagrange had communicated important discoveries to Euler; and to be Professor of the Elements of Mathematics at sixteen, he had no occasion for any of those analytical works which he began to read at the age of seventeen.—*Note by the Editor of the Moniteur.*

† What rendered these details obscure was the necessity of abridging them for the public reading of the article, in consequence of which at least a third of the whole was omitted. At first the method of variations, and the principle of least action, were named separately; and in a note the two Latin passages given in the text were cited, one relative to $P\,d\,p$, the other to the metaphysical principle.—*Note by the Editor of the Moniteur.*

would wish to see in a work of pure analysis. The author admitted this himself. He allowed the necessity of a demonstration independent of geometry. " He appeared to doubt the resources of analysis, and terminated his work by saying, If my principle be not sufficiently demonstrated, yet as it is conformable to truth, I have no doubt that by means of a rigid metaphysical explanation it may be put in the clearest light, and I leave that task to the metaphysicians.

" This appeal, to which the metaphysicians paid no attention, was listened to by Lagrange, and excited his emulation." In a short time the young man found the solution of which Euler had despaired. He found it by analysis. " And in giving an account of the way in which he had been led to that discovery, he said expressly, and as it were in answer to Euler's doubt, that he regarded it not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary from a more general law which he afterwards made the foundation of his Mechanique Analytique."— (See that work, p. 189 of the first edition.)*

Let us now give some exact details.

Euler, in his famous work on isoperimetres (Methodus Inveniendi, &c.) expressed a wish that a purely analytical solution of the general question could be found. He expresses himself as follows in the 56th page of that work : " Desideratur itaque methodus a resolutione geometrica et lineari libera, qua pateat in tali investigatione maximi minimive, loco $P\,d\,p$ scribi debere $-\,p\,d\,P$." This was the appeal to which Lagrange listened, and which he answered by the discovery of the method of variations. He has said nothing respecting the ideas which led to the discovery. It was not published till 1762, though he had communicated it to Euler by letter as early as 1755. (See the Melanges de Turin, t. iv. p. 163.)

On the other hand, at the end of his work on isoperimetres, Euler had introduced two appendixes ; one on elastic curves, the other on the motion of projectiles. In this last he demonstrated that " in the trajectories described by central forces the integral of the velocity multiplied by the element of the curve is always either a *maximum* or a *minimum*." But he only perceived this property in the motion of isolated bodies, and made vain attempts to extend it to the motion of those which act upon each other in any manner whatever. All he could do was to satisfy himself, by a metaphysical argument, that it ought to apply in these cases also ; and he terminated his dissertation in the following manner : " Cujus ratiocinii vis, etiamsi nondum satis perspiciatur, tamen quia cum veritate congruit, non dubito quin, ope principiorum sanioris metaphysicæ, ad majorem evidentiam evehi queat ; quod negotium aliis, qui metaphysicam profitentur, relinquo."

Lagrange, being in possession of the method of variations, did

* In transcribing these paragraphs the passages that will not apply to the method of variations have been marked with inverted commas.

not fail to apply it to this question; and when he informed Euler in 1756 that he had succeeded in extending his fine theorem to any system whatever of bodies, he made him acquainted with the method of using it to resolve all the problems in dynamics. (See the Melanges de Turin, t. iv. p. 166.) We see likewise, by his prize essay on the Libration of the Moon, that in 1764 he had already perceived that this pretended *principle of least action* was only a consequence of the principle of *virtual velocities.* But it was only in his Mechanique Analytique that he demonstrated this consequence; and it is there only that he says, without giving any account of the way by which he was led to the discovery, that he regards the principle of the least action, not as a metaphysical principle, but as a simple and general result of the laws of mechanics. (P. 189.)

I shall point out still two slight inaccuracies in the biographical account of Lagrange.

It is said that the concert of praises of which Lagrange was the object was interrupted only *one single time.* It would have been better to have said, by *one single man*; for Fontaine attacked Lagrange in the Memoirs of the Academy two different times: in 1767, on the method of variations; and in 1768, on the solution of the problem of tautochrones. Lagrange made two separate answers. He answered the first attack in the fourth volume of the Melanges de Turin, and the second in the Berlin Memoirs for 1770.

It is stated likewise that the lectures of Lagrange at the Polytechnic School, published under the well-known title of a Theory of Functions, are the developement of the ideas contained in two memoirs published in 1772. But he only wrote one memoir on that subject. It is to be found in the Berlin Memoirs for 1772. It may be proper on this occasion to mention, that Arbogast, in a paper sent to the Paris Academy in 1789, anticipated Lagrange in the application of geometry to the principal idea of this paper of 1772.

After these observations, for the most part of little importance, we will proceed to the supplement which we promised, and state faithfully what a long intimacy enabled us to collect from the conversation of this great mathematician.

A striking expression of his is noticed in the biographical account. " Had I been possessed of a fortune," says Lagrange, " in all probability I should not have devoted myself to mathematics." He must have regarded such an obstacle as a real advantage; for I remember one day when a young man was presented to him, who devoted himself to mathematics with peculiar ardour, his first question was, " Are you possessed of a fortune?" and when the answer was not a denial, " So much the worse," replied Lagrange, " the want of fortune, and of the consequence which it gives in the world, is a constant stimulus for which nothing can be substi-

tuted, and without which the necessary attention is not paid to such severe labours as the mathematics require." The event justified the truth of this prognostic.

He was alarmed for those who aspired at eminence in the science of analysis, on account of the immense progress which it had made since the period of his first studies. He said on one occasion, with that naiveté which rendered him no less interesting than his genius, pointing to a pile of modern books lying on his table, " I pity the young mathematicians who have so many thorns to wade through. If I were to begin again, I would not study. These large quartos frighten me too much." He added soon after, " It is needless to accumulate books; true lovers of mathematics will always read Euler, because in his writings every thing is clear, distinct, and correct; because they swarm with excellent examples; and because it is always necessary to have recourse to the fountain head."

Whenever any body spoke before him of a new edition of a mathematical book, he expressed a wish that the original treatises on the infinitesimal calculus were printed in one volume; namely, the method of Fermat for the *maxima* and *minima*; the memoir of Leibnitz which contains the explanation of the differential calculus (Leipsic Acts for 1684); l'Hopital's treatise on infinitely small quantities; and John Bernoulli's lectures on the integral calculus. He held these lectures in very great estimation; and said that he was particularly indebted to the study of them, especially because when a youth they were only lent to him, and on that account he was obliged to make himself quite master of them. This collection of the works of the first inventors in all their purity pleased his imagination.

He wished likewise to see a collection formed of some memoirs of a later date, such as those of Euler on the movements of rotation (Mem. Berlin, 1758), that of d'Alembert on some methods of the integral calculus (Mem. Berlin, 1748), &c. He admired, particularly in the last mentioned memoir, the ingenious artifice of the author to avoid the difficulty which occurs in the case of equal roots in linear equations; and often remarked, with some bitterness, that the world seemed to be gradually forgetting how much mathematics owed to the genius of this great geometer. " From my earliest studies," said he, " I had imbibed the greatest admiration for d'Alembert, and I have always preserved it, because it was he who in the last age made the greatest number of brilliant discoveries. I acknowledge, however, that Euler will always be studied in preference by the greater number of persons, and with reason, because he is a better writer. These are my two great men," added he, " the two whom I esteem most after Newton; but every person cannot be so fortunate as Newton was." Accordingly during the French revolution when he burnt almost all his papers and letters, those of d'Alembert and Euler alone escaped this necessary proscription.

I think after this I may be permitted to doubt whether Lagrange

would have entirely approved of the expression of the author of his life, who describes Euler as *holding the sceptre of geometry* at the time when Lagrange began his mathematical career. I shall quote likewise some paragraphs from Euler's letters, to show that he was not disposed to arrogate to himself so much superiority over his rival. They were written from Berlin to a French philosopher, in 1752, and the succeeding years.

" M. d'Alembert has a wonderful fertility of invention, as is obvious from every thing that he does. His most important work is his essay on the precession and the nutation ; and that question is one of the most difficult that can be discussed."

" I feel disgusted with labouring at the theory of the moon, especially since M. d'Alembert has said that he has a particular way of treating the approximations, and of neglecting very little. I wish I knew the smallest portion of his method. I would undertake the labour of discovering the whole."

" I am much obliged to you for the information you have given me respecting the work of d'Alembert on the moon, for the publication of which I wait with the greatest impatience. The more I have examined the difficulties of this subject, the more I am convinced that nobody is able to overcome them but M. d'Alembert, whose penetration fills me with as much admiration as esteem." *

Let us remember that these opinions were given, by so great a man, of him who *first* gave a general method of reducing all questions about motion to questions about equilibrium ; who discovered the principles of the motion of rotation of solid bodies, and rigorous equations respecting the motion of fluids ; introduced into mechanical philosophy the calculus of partial differences ; resolved with the necessary rigour the problems of vibrating cords, of the precession of the equinoxes and the nutation; and treated generally the figure of the planets—of him who invented the method of integrating simultaneous equations, and various other contrivances in the integral calculus no less useful than ingenious—of him whose solution of the problem of three bodies is preferred by the illustrious author of the Mecanique Celeste, and who threw a philosophical eye on all the bases of pure and mixed mathematics. If we recall all these true titles of glory, we may satisfy ourselves with laughing at the English journalist† who speaks of d'Alembert as a plagiary ; but for the honour of the nation, we must regret that Frenchmen, speaking of his famous principle of dynamics, have

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* These fragments make us acquainted with the opinion which Euler entertained of d'Alembert as a mathematician. The following shows us what he thought of him as a man :—" I had the honour of seeing here M. d'Alembert, who showed me so much friendship that I am filled with gratitude, and should be highly delighted were he to accept the place of President of our Academy. It would be the only method of doing us honour here. I flatter myself that he will be at last induced to yield to our solicitations. At present my eldest son is very well established ; by the recommendation of M. d'Alembert, the King has given him a pension of six hundred crowns," &c. Written in 1763.—*Note by the Editor of the Moniteur.*

† The Quarterly Review.

employed the expression "principle *ascribed* to d'Alembert," as if it did not belong to him with more justice than that of virtual velocities belong to Lagrange himself; as if the solution of the problem of the centres of oscillation by James Bernoulli, in which we find an analogous idea employed, had contained that principle so clearly that we ought to ascribe the discovery of it to him! Yet that solution was for forty years in the hands of others, of Bernoulli, Taylor, Cotes, Maclaurin, Fontaine, Clairaut, and Euler himself, who were continually proposing particular questions in dynamics, without ever thinking of this famous principle; and when once d'Alembert published it, all these mutual attempts ceased for ever.

Let us return to Lagrange, from whom this long digression has perhaps drawn us too far.

In speaking of the *good fortune* of Newton, who had found a *system of the world to explain* (a good fortune, he observed, with a serious and almost melancholy tone, which we do not meet with every day), he took a pleasure likewise in pointing out what he called the good fortune of one of his associates, whose inventive and original genius had greatly struck him.* I shall venture likewise to state an observation of his on a similar subject, which gives a faithful picture of his manner of expressing himself when he was strongly penetrated with his subject: " See," said he, one day, " that confounded * * * * * with his application of analysis to the generation of surfaces; he will be immortal, he will be immortal !"†

His candour was equal to his penetration; and the continual contrast of these two qualities gave to his company a high degree of interest. As his ideas were always perfectly precise, he always wished that the expression of them should be a faithful picture of his conceptions. Hence when he had begun a phrase which he despaired of finishing clearly, those original interruptions, usually followed by his favourite word, *I do not know, I do not know,* without attempting to finish the sentence, he left it abruptly. Often also his unlooked for silence was occasioned by a new idea which came across him, and which rapidly absorbed his faculties of thinking. Who has not seen him interrupt, all of a sudden, the lecture which he was giving at the Polytechnic School, appear sometimes embarrassed like a beginner, quit the table, and sit down opposite to the audience, while masters and scholars, confounded on the benches, waited in a respectful silence till he brought back his thoughts from the spaces through which he had allowed them to wander!

Real abilities always obtained his suffrage; I had almost said his homage, such was his modesty. He always spoke of his predecessors in the career of mathematics, and of those who had

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* I presume the person here alluded to is Lavoisier. —T.
† I conceive the person alluded to by Lagrange is Monge. —T.

gained a deserved reputation in our own time, with the highest esteem and regard. Condorcet alone was held in no estimation by him. He said that this academician " had done nothing tolerable except his first work ; that his other productions were indifferent or bad ; that he had not during his whole life integrated a new equation ; that he would have destroyed analysis if he had been let alone ; and that in his hands it became a complete barbarism."

He reproached him, as well as Fontaine, for having sought for direct methods of integration, and appeared convinced that it was a downright absurdity to expect to discover such. He blamed d'Alembert likewise for having too carelessly discarded the question of living forces, by affirming that it was merely a question about words, and proposed to elucidate more completely that famous discussion.

When he spoke of the system of the world, one of his favourite remarks was the disparity between some of the constant quantities representing the orbits, and other elements. " Nature," said he, " seems to have disposed these orbits on purpose to enable mathematicians to calculate them. Thus the eccentricity of the planets is very small, and that of the comets enormous. Without this disparity, so favourable for approximations, and unless the constants were of a moderate greatness, farewell to mathematicians ; it would be impossible to do any thing."

On some occasions he showed uneasiness at the imperfection of the methods of approximation employed in physical astronomy, and seemed to fear that they would become a kind of mine from which any one might draw whatever he thought proper. But these doubts were stated in a low voice, and always accompanied with his favourite " I do not know."

He regretted not having returned more frequently upon some parts of his works which exhibited errors to correct, or omissions to supply. " I did not do it," he said, " because it was the practice of d'Alembert, and subjected him to ridicule. I allowed myself, in consequence, to be deprived of a great many important discoveries, which followed from my memoirs."

Of all his works, that which he himself seemed to value most highly was his paper on the integral calculus of irrational functions, published in the Turin Memoirs for 1784. But he was not in the habit of speaking of his own works ; and his modesty prevented him from recommending the study of them.

In consequence of his great reputation, he was often consulted by those who wished to study mathematics, and who were of opinion that he could easily point out to them the best method of regulating their studies. But he disliked giving advices of that nature. He had himself studied without any assistance from others, and hence he thought that other men were endowed with the same capacity and industry as himself. His usual answer was, that in geometry no master is necessary, and that nothing is well learned but what a man learns by himself. When his opinion was still persisted in, " Study Euler," he would say, " and make a point of

resolving the problems which you will meet with in him ; for when you merely read the solutions of another, you will neither perceive the reasons which induced him to turn to this side or that, nor the difficulties which he met with in his passage."

One day, while speaking to me of his dislike to give directions about one mode of study rather than another, he ascribed it to his never having had a master nor companion in his labours ; in consequence of which he never had an opportunity of investigating that subject : " Not," said he, " that I could not speak of it as well as another ; for I reflected at an early age upon the best method of proceeding in the study of analysis, and I laid down a certain number of principles, which I have always faithfully followed, and which I will mention to you.

" I never studied more than one book at a time ; but if it was a good book I read it to the end.

" I did not perplex myself with the difficulties, but left them to return to twenty times in succession if necessary. If after all these efforts I could not make them out, I examined how another mathematician treated the same point.

" I never quitted the book I had chosen without understanding it thoroughly, and I passed by every thing with which I was well acquainted when I met with it again.

" I considered reading large treatises of pure analysis as quite useless. Too great a variety of methods pass at once before the eyes. It is in the works in which they are applied to use that we ought to study these methods. We can estimate their utility, and we learn the method of using them. In my opinion we ought to devote our time and our labour chiefly to the applications. In general we should satisfy ourselves with consulting the great works on the calculus, unless we find in them methods either new or curious on account of their analytical uses.

" During my reading I reflected chiefly on what could have led my author to such or such a transformation or substitution, and on the advantage which resulted from it. After this I examined whether some other would not have answered better, that I might become expert in this great means of analysis.

" I always read with the pen in my hand, developing all the calculations, and exercising myself on all the questions that I found ; and I considered it as an excellent practice to make an analysis of the methods, and even an extract of the results, when the work was important, or highly esteemed.

" From the very beginning of my career I endeavoured to make myself master of certain subjects, that I might have an opportunity of inventing improvements ; and I always, as far as possible, made *theories to myself* of the essential points, in order to fix them more completely in my mind, to render them my own, and to accustom myself to composition ?

" I took care to recur frequently to mathematical considerations, which I considered as very proper to give force and correctness to the judgment.

" Finally, I always took care every day to assign myself a task for the next. The mind is indolent. We must get the better of this natural inactivity, and keep our industry in full vigour, in order to be able to exert all our powers when occasion requires them. Nothing but exercise can accomplish this. It is likewise an excellent practice to accustom oneself as much as possible to do the same things at the same hours, reserving the most difficult for the morning. I learned that custom from the King of Prussia, and I have found that this regularity gradually renders labour more easy and more agreeable." *

As to his fourth principle, I may state that when he was scarcely acquainted with the first rudiments of the differential and integral calculus, he undertook the perusal of Euler's Mechanics, in which he not only learned dynamics, but likewise the integral calculus, properly so called; and he assured me that this labour had greatly improved his mathematical skill. The fine problems with which that book is filled greatly facilitated the perusal of Newton's Principia, the study of which he combined with that excellent work. " Read it therefore with care," he would say, " as well as the beautiful theory of the motion of solid bodies, which follows it." Then yielding to his admiration of Euler, he placed him at the head of all who have written upon mathematics for clearness, method, and for the beautiful examples which recur without end. He finished by repeating, " If you wish to be a mathematician, you must study Euler." Every other person would have said, EULER and LAGRANGE.†

* I can assure the reader that all these statements were communicated to me by M. Lagrange one evening, the first part of which he had employed in assuring me again that he did not like to talk about the method of studying the mathematics. He had sometimes the custom of making such professions, and then gradually entering upon the subject that he disclaimed, provided his hearers took great care not to appear themselves to be sensible of his procedure, but allowed him to be carried away by the momentary impulse which directed him. During an intimacy of fifteen years, during which I was very frequently in his company, such a conversation took place only once. I listened to him with more than usual avidity, and I took care, when I returned home, to write down a faithful abstract of the whole conversation.

† Mathematicians, and, above all, those who are endeavouring to become mathematicians, must be anxious for the publication of a collection of the most important dissertations of Lagrange. His principal treatises on analysis, mechanics, and the system of the world, are scattered through various academical collections, which it is difficult, and sometimes impossible, to procure. Besides, it is very inconvenient to have to turn over thirty or forty volumes for what might easily be united in three or four. The author of this notice can assure his readers that in such a space might be united about thirty-five of the most rare and most important papers of Lagrange. He would with pleasure set about the publication, if he thought that it would be encouraged by the lovers, so numerous at present, of the most perfect of all the sciences. If their wishes should reach him, he will put into the hands of a bookseller a list of the memoirs, accompanied with the reasons for selecting them, and with the classification to which it is conceived they ought to be subjected.

Article II.

On the Daltonian Theory of Chemical Proportions.
By Thomas Thomson, M.D. F.R.S.

(*Continued from* vol. iii. p. 378.)

I HAVE hitherto avoided noticing muriatic acid and chlorine on account of the difference of opinion which still exists respecting the nature and composition of these bodies.   But as muriatic acid is more used by chemists than almost any other re-agent, the knowledge of the constituents of the compounds which it forms is of the utmost consequence.   On that account I have been induced to wave that delicacy which the handling of a controversial subject requires, and to sacrifice etiquet to utility.

As all attempts to decompose chlorine, and to obtain oxygen gas from it, which have been made by Gay-Lussac and Thenard, Davy, and various other persons, have entirely failed, I do not see how it is possible to avoid considering it, in the present state of our knowledge, as a simple substance.   If we neglect this rule, namely, to deduce all our principles in chemistry from experiment, and never to proceed farther than we are warranted by experiment, we cannot possibly avoid going astray, and filling the science with hypotheses and absurdities.   I think it very likely, from the great weight of the atoms of the metals, of chlorine and iodine, that they are all compounds ; but we cannot introduce any such opinion into the science, far less make it the foundation of our reasoning and deductions, till some evidence be brought forward from experiment that our opinion is well founded.   Analogical reasoning is always dangerous ; and in no science is it so apt to mislead as in chemistry.   Lavoisier's hypotheses respecting the acidifying principle, and the composition of the alkalies, furnish very striking proofs of the truth of this proposition.

I have carefully considered the arguments advanced by Mr. Murray, Professor Berzelius, Mr. Henderson, and the French chemists, in support of the opinion that chlorine is a compound of muriatic acid and oxygen ; but I do not think that they have been able even to establish an analogy in favour of their opinion.   Mr. Murray's papers are all written with a clearness, force, and acuteness, that does him great credit.   Indeed, nothing can afford a more striking proof of his abilities than the plausible point of view in which he has contrived to place an hypothesis in favour of which not a single decisive experiment, or even analogy, can be adduced. In some particulars he has even the advantage in the argument ; an advantage which it would have been easy to have anticipated from Sir Humphry Davy's turn of mind: for the merit of this illustrious chemist by no means consists in the minute precision of his experi-

ments; but in the ingenuity of his contrivances, and the happy analogies by which his experiments are directed.

The arguments of Professor Berzelius are entirely analogical; and, as far as I can perceive, not in the least hostile to the opinion of Sir Humphry Davy : for the theory of definite proportions, as I shall show in this paper, applies as accurately to the one hypothesis as the other. Mr. Henderson's arguments are of the same kind with those of Professor Berzelius ; and he has been misled in almost all his examples, by adopting inaccurate data (though the best that could be found) respecting the composition of the different substances.

The French chemists (I allude to Gay-Lussac and Thenard) seem to me to have been aware of the lameness of their arguments, and to have adopted the old opinion merely as a compliment to Berthollet, whose reputation was chiefly raised by his original experiments on chlorine, and his supposed analysis of it.

I am surprised that the assertion of Gay-Lussac and Thenard, that muriatic acid gas contains more than a fifth of its weight of water, did not startle the chemical world in general. No analogy can be adduced in favour of such an opinion, but abundance against it. The experiment of Dr. Bostock and Dr. Trail, repeated by Mr. Murray, by Sir H. Davy, and by Dr. Hope, is quite inconsistent with such an opinion. I am surprised that any stress should be laid upon the appearance of a minute portion of water when salammoniac is formed by the union of muriatic acid and ammoniacal gases. Whoever will attend to the experiments of Dr. Henry will perceive the extreme difficulty, if not impossibility, of excluding all moisture in such cases.

I think then that in the present state of our knowledge we have no other alternative than to adopt the opinion that chlorine is a simple supporter of combustion analogous to oxygen and iodine, and capable, like them, of combining with the different combustible bases, and of forming a class of bodies analogous to the oxides and acids constituted by the union of oxygen to the same bases. I consider Sir H. Davy's mode of naming these compounds as very unfortunate. It is not only contrary to the analogy of our language, and of all the languages of Europe, except perhaps the Italian; but it introduces into chemistry above forty new words, and therefore greatly increases the difficulty of becoming acquainted with the science. I think on these accounts there can be no hesitation in rejecting it. There is a very obvious method of naming these compounds, suggested by the term *oxide*. We have only to apply to them all the appellation *chloride*, and distinguish each species by adding the name of the base. Thus *chloride of sulphur* will be my muriate of sulphur, *prochloride of phosphorus* and *perchloride of phosphorus* will be the *phosphorane* and *phosphorana* of Davy, *chloride of silver* will be *horn silver*, or *muriate of silver*, and so on.

The chlorides of the simple combustibles are all very satisfactorily

made out; but there is still an obscurity hanging over the chlorides of the metals. There can be no doubt that muriatic acid has the property of combining with metallic oxides, and forming salts; for if you dissolve iron or zinc in muriatic acid, you obtain just the same bulk of hydrogen gas as if you dissolve the same weight of these metals in sulphuric acid. Now as the hydrogen in the last case comes from the decomposition of water, I do not see how we can hesitate to ascribe it in the first case to a similar decomposition; and then all the rest follows of course : but on the other hand, when sodium and chlorine combine directly, we obtain a substance possessing exactly the properties of common salt. The same observation applies to all the metals. Hence I do not see how any other explanation of these combinations can be given than that which Davy has offered; though I must own that I am far from being satisfied with it, as it supposes the decomposition and composition of water with a degree of facility much greater than has been hitherto observed.

I shall now give a table of the composition of such of the chlorides as have been made out with tolerable exactness. Indeed, from the great insolubility of *chloride of silver*, I conceive that these analyses are in general more accurate than those of any other class of bodies, except the sulphates.

	Number of atoms.	Weight of an integrant particle.
251 Chlorine		4·198 [a]
252 Chloride of oxygen (*euchlorine*)	1 *ch* + 1 *o*	5·498 [b]
253 Muriatic acid	1 *ch* + 2 *h*	4·762 [c]
254 Chloride of sulphur	1 *ch* + 1 *s*	6·498 [d]

[a] This number results from the supposition that the euchlorine gas of Davy is composed of one atom of chlorine and one of oxygen, and that the specific gravity of chlorine gas is 2·483, which I find to be nearly the truth. Sir H. Davy's number is 4·466, which does not differ much from mine. Dr. Wollaston's number is 4·410. The theory of volumes would lead us to adopt half my number as the weight of an atom of chlorine. It is impossible to say which hypothesis is correct : but the number I have chosen exhibits the simplest view of the composition of the chlorides.

[b] This gas is composed of two volumes of chlorine gas and one volume of oxygen gas.

[c] This supposes muriatic acid to be composed of equal volumes of chlorine and hydrogen, which agrees with experiment.

[d] According to Davy, 10 grains of sulphur absorb 30 cubical inches of chlorine. Now 30 cubic inches of this gas weigh 22·719 grains. Now $1 : 2·2719 :: 2 : 2·2719 \times 2$. And 4·5438 differs but little from the weight of an atom of chlorine.

	Number of atoms.	Weight of an integrant particle.
255 Prochloride of phosphorus....1 ch + 1 p		.... 6·241 [e]
256 Perchloride of phosphorus....2 ch + 1 p		....10·996 [e]
257 Chloride of azote ....4 ch + 1 a		....19·795 [f]
258 Chloride of boron. Unknown.		
259 Chloride of carbon. Unknown.		
260 Chloride of potassium ......1 ch + 1 p		.... 9·498 [g]
261 Chloride of sodium ....2 ch + 1 s		....14·878 [h]
262 Chloride of ammonium .....1 ch + 1 am		.... 5·647 [i]

[e] The first of these is the liquid obtained by passing phosphorus through corrosive sublimate. As it has never been analysed, the composition is merely stated from analogy. The second is the white powder obtained by burning phosphorus in a maximum of chlorine. According to Davy, it is composed of 1 phosphorus + 6·8 chlorine. This comes out nearly $2\frac{1}{2}$ chlorine + 1 phosphorus ; but I have little doubt that the proportion of chlorine is over-rated, and that it is really a compound of two atoms chlorine and one atom phosphorus.

[f] This corresponds with the result of Davy's analysis, who found the substance in question a compound of four volumes of chlorine and one volume of azote ; but very little confidence can be placed in the analysis.

[g] According to this statement the chloride of potassium (*muriate of potash*) should be a compound of 100 chlorine + 111·15 potassium. Now Dr. Wollaston gives us 100 chlorine + 111·337 from the experiments of Klaproth ; and the analysis of Berzelius gives us 100 chlorine + 112·48 potassium (Ann. de Chim. lxxix. 139). Both of these results agree almost exactly with the numbers in the table.

[h] According to this statement, chloride of sodium (*common salt*) ought to be composed of 100 chlorine + 62·05 sodium. Now Dr. Wollaston gives us its composition from Davy, 100 chlorine + 65·986 sodium.   Berzelius's analysis makes it 100 chlorine + 66·869 sodium (Ann. de Chim. lxxix. 140).

[i] From the properties of salammoniac there can be little doubt, I think, that it is a compound of chlorine and ammonium. It is not decomposed by heat, which is not the case with any other ammoniacal salt. I ascertained long ago that muriate of ammonia is composed of equal volumes of muriatic acid gas and ammoniacal gas.   Hence it consists of

Muriatic acid 100
Ammonia .... 46·178
                              ————
By weight ....146·178

	Number of atoms.	Weight of an integrant particle.
263 Chloride of barytium ....1 *ch* + 1 *b*		....13·229 [k]
264 Chloride of strontium ......1 *ch* + 1 *st*		....10·398 [l]
265 Chloride of calcium ....1 *ch* + 1 *c*		.... 7·118 [m]
266 Chloride of magnesium .....1 *ch* + 1 *m*		.... 5·866 [n]
267 Chloride of silver ....1 *ch* + 1 *s*		....18·212 [o]
268 Prochloride of mercury.....1 *ch* + 1 *m*		....29·498 [p]

Now muriatic acid is a compound of 75·731 chlorine + 2·23 hydrogen. Therefore 100 muriatic acid contains 2·86 hydrogen. We must suppose this hydrogen to find in the ammonia a quantity of oxygen capable of converting it into water. Now 2·86 hydrogen require 21⅔ oxygen to convert them into water. Therefore ammonia must be a compound of

Ammonium ....24·511..or..100
Oxygen ....21·666..or.. 88·39

And chloride of ammonium is a compound of 97·14 chlorine + 24·511 ammonium.

[k] This supposes chloride of barytium (*muriate of barytes*) to be composed of 100 chlorine + 194·115 barytium. Now the analysis of Berzelius gives us 100 chlorine + 197·28 barytium.

[l] According to this statement chloride of strontium (*muriate of strontian*) is a compound of 100 chlorine + 131·17 strontium. Now analysis gives 100 chlorine + 133·78 strontium.

[m] According to this statement, chloride of calcium (*calcined muriate of lime*) ought to be composed of 100 chlorine and 58·25 calcium. Now Dr. Marcet's analysis gives 100 chlorine + 57·732 calcium, and Professor Berzelius's 100 chlorine + 58·923 calcium. The mean of the two gives 100 chlorine + 58·32 calcium, which almost coincides with the theoretic number.

[n] These are the theoretic numbers. We might deduce an experimental result from some of the analyses already made of muriate of magnesia; but as it would involve several hypothetic particulars I have not done it.

[o] If we suppose the weight of an atom of silver to be 12·618, as I have made it in a preceding part of this table: then *horn silver* would be a compound of 100 chlorine and 280·5 silver: but this does not agree with experiment. Wenzel found it a compound of 24·67 chlorine + 75·33 silver, Rose and Bucholz of 25 chlorine and 75 silver, Mr. John Davy 24·5 chlorine and 75·5 silver, and Berzelius 24·623 chlorine + 75·377 silver. The mean of these experiments supposes this chloride a compound of 100 chlorine + 304·89 silver. To obtain the same theoretic result we must suppose an atom of silver to weigh 13·714, and the oxide of silver to be composed of 100 silver + 7·291 oxygen.

[p] According to this statement, prochloride of mercury (*calomel*) should be composed of 100 chlorine + 555·8 mercury. We have

	Number of atoms.			Weight of an integrant particle.
269 Perchloride of mercury	2 ch	+	1 m	34·996 q
270 Prochloride of copper	1 ch	+	1 c	12·498 r
271 Perchloride of copper	2 ch	+	1 c	16·996 s
272 Prochloride of iron	2 ch	+	1 i	16·139 t
273 Perchloride of iron	4 ch	+	1 i	25·135 u
274 Prochloride of tin	2 ch	+	1 t	23·701 x

four analyses of this compound. According to Davy, it is composed of 100 chlorine + 567·16 mercury; according to Chenevix, of 100 chlorine + 576·14 mercury; according to Berzelius, of 100 chlorine + 589·65 mercury; and according to Zaboada, of 100 chlorine + 634·22 mercury. The difference between the theoretic result and Davy's analysis is only 2 per cent. The others deviate farther.

q This makes perchloride of mercury (*corrosive sublimate*) a compound of 100 chlorine + 277·9 mercury. Now Davy's analysis gives 100 chlorine + 283·58 mercury. Chenevix gives 100 chlorine + 332·15 mercury : but this result is certainly erroneous.

r This is the substance called *resin of copper* by Boyle, *white muriate of copper* by Proust, and *cuprane* by Davy. According to the numbers in the table, it is a compound of 100 chlorine + 177·85 copper. Now Mr. John Davy found 100 chlorine + 177·777 copper.

s This is a yellow powder, which may be obtained by evaporating green muriate of copper on a water bath. According to the table, it is a compound of 100 chlorine + 88·925 copper. Now Mr. John Davy's analysis gives us 100 chlorine + 89·286 copper.

t This is a greyish brilliant substance, obtained by heating green muriate of iron to redness. According to the statement in the table, it ought to be a compound of 100 chlorine + 79·402 iron. Mr. John Davy found it a compound of 100 chlorine + 87·16 iron; but the analysis was made upon too small a scale to be entitled to confidence.

u This is a volatile substance, obtained by burning iron wire in chlorine gas, or by heating red muriate of iron. According to the statement in the table, it ought to be a compound of 100 chlorine + 39·701 iron. Mr. John Davy found it composed of 100 chlorine 54·08 ; but his analysis is not entitled to confidence, from the minuteness of the scale. I consider an atom of iron to weigh 7·143, which later experiments induce me to prefer to the weight 6·666, given in a preceding part of this table.

x This substance may be obtained by distilling calomel and amalgam of tin, or by fusing the promuriate of tin. It is grey, and has a resinous appearance. According to the statement in the table, it ought to be a compound of 100 chlorine + 163·46 tin. Now Mr. John Davy's analysis gives 100 chlorine + 164·69 tin.

	Number of atoms.	Weight of an integrant particle.
275 Perchloride of tin ....4 ch + 1 t		....32·697 [y]
276 Chloride of lead ....2 ch + 1 l		....34·970 [z]
277 Chloride of zinc ....1 ch + 1 z		.... 8·637 [a]
278 Chloride of bismuth ....1 ch + 1 b		....13·493 [b]
279 Chloride of antimony .....2 ch + 1 a		....20·107 [c]
280 Chloride of arsenic.........2 ch + 1 a		....14·996 [d]
281 Chloride of manganese......2 ch + 1 m		....16·126 [e]
282 Chloride of carbonic oxide...1 ch + $c.ox$		.... 6·249 [f]

[y] Perchloride of tin (*fuming liquor of Libavius*), according to this statement, is a compound of 100 chlorine + 81·73 tin. Now Mr. John Davy's analysis gives 100 chlorine + 82·089 tin.

[z] This is obtained by fusing muriate of lead. According to the statement in the table, it ought to be a compound of 100 chlorine + 288·725 lead. Now Mr. John Davy obtained by analysis 100 chlorine + 287·9 lead.

[a] This is obtained by fusing dry muriate of zinc. According to the statement in the table, it ought to be a compound of 100 chlorine + 92·019 zinc. Mr. John Davy's analysis gives 100 chlorine + 100 zinc. This does not correspond quite so well as most of the other analyses of that gentleman with the theoretic result.

[b] It is obtained by distilling a mixture of bismuth and corrosive sublimate. According to the statement in the table, it is composed of 100 chlorine + 199·98 bismuth. Mr. John Davy obtained by analysis 100 chlorine + 197·57 bismuth.

[c] This is the butter of antimony. If it be composed as stated in the table, it consists of 100 chlorine + 123·51 antimony. Mr. John Davy's analysis gives 100 chlorine + 152·65 antimony. Probably my number for the weight of an atom of antimony is inaccurate.

[d] This is the *butter of arsenic,* formed by distilling a mixture of corrosive sublimate and arsenic, or by burning arsenic in chlorine gas. According to the statement in the table, it is a compound of 100 chlorine + 66·669 arsenic. Mr. John Davy's analysis makes it a compound of 100 chlorine + 56·88 arsenic ; but he makes the weight of 4 cubic inches of chlorine gas 3·06 grains, whereas they weigh only 3·03 grains. The correction of this error makes his result 100 chlorine + 66·05 arsenic, which almost agrees with the tabular statement.

[e] This is obtained by fusing the white muriate of manganese. According to the statement in the table, it is a compound of 100 chlorine + 79·26 manganese. Mr. John Davy's analysis gives us 100 chlorine + 85·185 manganese; but I consider it as in some respects objectionable.

[f] This is the phosgene gas of Mr. John Davy. According to the

I shall not attempt to carry this table of chlorides any farther. It would not be difficult to add a few more from the analyses of the muriates of those metals that have been omitted ; but I do not consider these muriates as analysed with sufficient precision to found upon them such important consequences. The reader may easily supply this deficiency by attending to the two following rules :— 1. If the muriate has been analysed by means of muriate of silver (which is the common way), to find the composition of the chloride divide the quantity of muriatic acid found by $3 \cdot 5$, add the quotient to the muriatic acid : the sum is the quantity of chlorine in the chloride. Subtract the same quotient from the metallic oxide. The remainder gives the quantity of metal in the chloride. 2. If the muriate has been analysed by obtaining the metallic basis in the metallic state, let the weight of metal thus found be a; then the chlorine $= 100 - a$.

There is a striking resemblance between the formation of the chlorides by means of muriatic acid and that of the oxides by means of nitric acid. Nor would there be any thing singular in the case were it not that most of these chlorides, when exposed to the air, absorb moisture, and are converted into muriates. Nothing analogous takes place with the oxides.

The number of chlorides contained in the preceding table is 29. Many, no doubt, remain unknown. Several of the most striking of them are liquids remarkable for their volatility, and capable of dissolving sulphur, phosphorus, oils, and resins. They are analogous to *oxides*, and probably capable, like them, of combining with acids and forming *salts* ; but we have no means of trying to form such combinations, except in a few instances ; because the chlorides cannot come in contact with water without undergoing decomposition. The acids that contain chlorine for their acidifying principle, as muriatic acid, chloride of carbonic oxide, are those that probably will be found to combine most readily with these chlorides. Indeed I think it very likely that with sufficient care a considerable number of these new saline bodies may hereafter be discovered, though it is not likely that many of them will be of much importance, as far as utility is concerned ; as we have reason to believe that water, or even simple exposure to the air, will decompose most of them.

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statement in the table, it is a compound of 100 chlorine $+ 38 \cdot 928$ carbonic oxide. Mr. John Davy's analysis, when corrected, gives us 100 chlorine $+ 38 \cdot 505$ carbonic oxide.

Plate IX

Fig. 1

Fig. 2

Fig. 3

Fig. 4

D.I. Wilkinson on the Doctrine of Refraction.
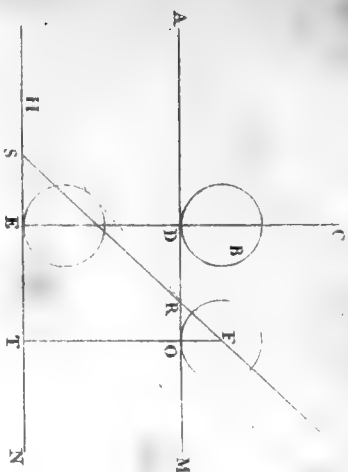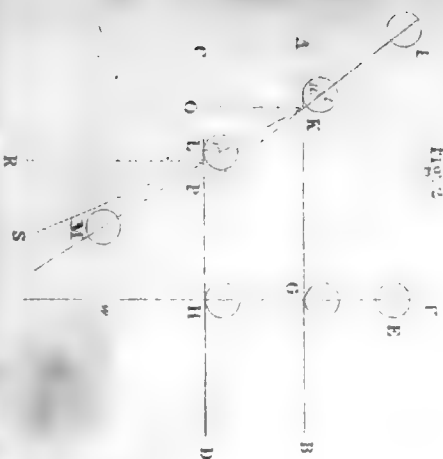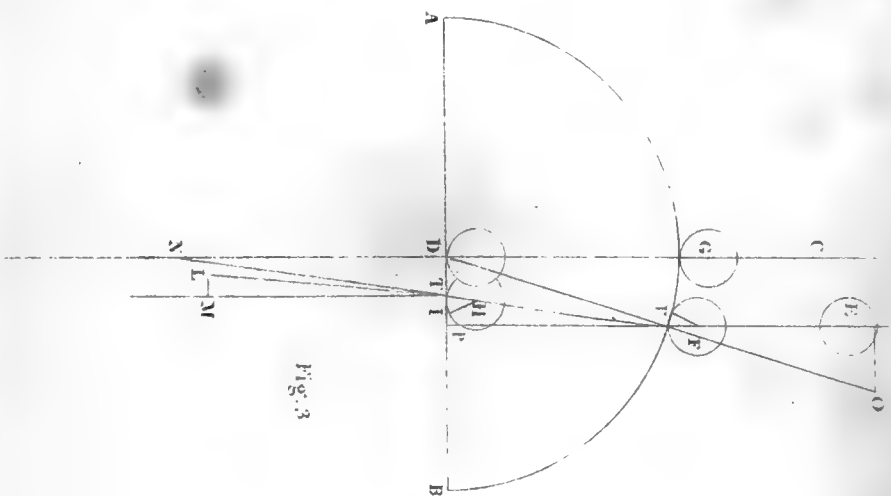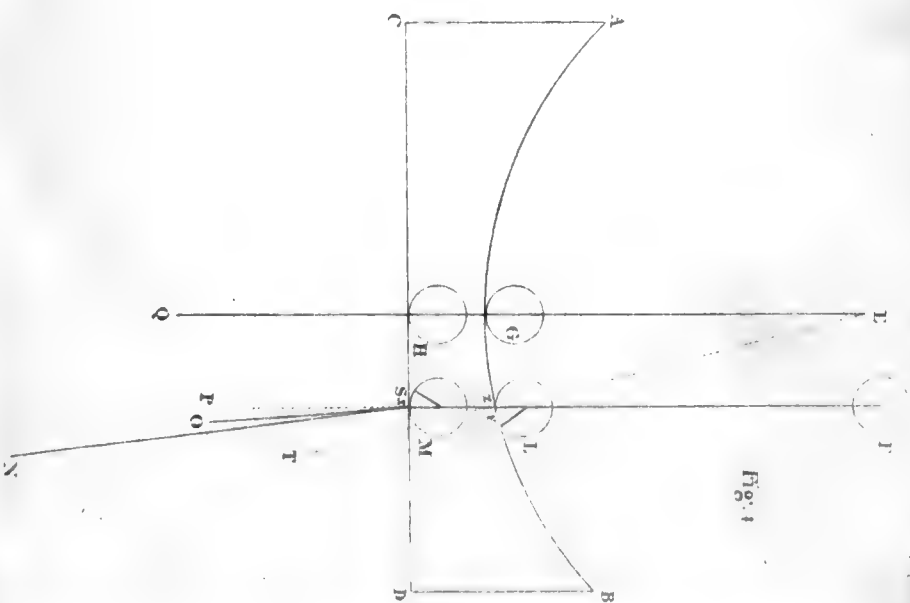
ARTICLE III.

An Explanation of the Doctrine of Refraction on Mechanical Principles. By C. H. Wilkinson, M. D.

(To Dr. Thomson.)

DEAR SIR,   Kingston House, Bath, May 5, 1814.

HAVING for many years, in the annual Course of Lectures I deliver in this city, been accustomed to attempt the explanation of those changes in the condition of inanimate substances, which have been referred to attractive and repulsive influences, to causes purely mechanical; if these conjectures should be worthy of insertion in your *Annals of Philosophy,* I shall be induced to trouble you with my further remarks.   I am, dear Sir,

Yours most respectfully,

C. H. WILKINSON.

Suppose A M H N, Plate XX. fig. 1, to represent a section of a reservoir of water; A M, the horizontal plane; let B represent a ball directed in the line, C E, perpendicular to the plane, A M; the point, D, where it will touch the plane, will be in the line of direction C E, entering into another medium; all its forces will conspire in the same line of direction to overcome the resistance, the velocity will be diminished in the ratio of that resistance, but the direction will be preserved uniform in the perpendicular line, C D E.

If the ball should be projected in the oblique direction, G F R, forming an angle, G R M, with the plane, A M, the ball will be in contact with the resisting medium at the point, O, forming the acute angle, O F R, the compliment of the angle of inclination of the ball with the horizon, or with the surface of the fluid, A M. The ball being more resisted at O than in any other part it becomes deflected from the direction G R S, and proceeds in the line R H, so that the angle S R H is the angle of deviation, induced by the resisting medium.

At whatever angle the ball be directed, provided it penetrates the medium, H T and S T will preserve a constant ratio.

It will be equally evident, that if the ball were propelled so as to pass out of one medium into another less resisting, that the angle of deviation would be reversed, so that the sine of the refractive angle would be less than the sine of the incident angle.

In the above example it is evident that the resistance will be in proportion to the density of the fluid: such will noways operate as an objection to the application of the same principle, to account for that deviation from the right line, of a particle of light, when determined out of one medium into another.

Let A B C D, fig. 2, represent any medium with parallel sides,

A B, C D; let E be a particle of light determined in the direction E G H, perpendicular to the plane, A B, it is evident that the contact of the particle of light with the plane will be in the line of its direction, *v w :* so likewise at the point of emersion, H, there will be a similar coincidence; hence there will be no deviation from the right line.

If the particle of light, I, be determined in the direction K I, forming an angle, K *t u*, with a perpendicular to the plane, the point *u* will be the first portion of the particle of light which touches the medium. In this medium it is less resisted than in the medium through which it has just passed : hence the deviation will be towards the perpendicular K O, instead of proceeding in the right line K P, it will describe the line K L ; in this case L K P will be the angle of deviation, whatever inclination may be the direction of the particle of light : in the same medium the sines of the angles of deviation and of right direction will bear a constant ratio, *i. e.* O L will be to O P in an uniform proportion.

When the particle of light arrives at the other side, C D, the point of contact is at *v*, and not at L ; in that point it is more resisted than in any other, consequently will be deflected from the perpendicular L R, and will form a corresponding angle of deviation, S L M ; and as A B and C D are parallel sides, it is very evident that the emergent particle of light must move in a direction, L M, parallel to its incident direction, I K.

The same principle readily applies to the direction of a particle of light through any curvilinear media. For this illustration I have selected a plano convex and a plano concave. The particle of light in both instances I have supposed as moving in a direction parallel to the axis of each medium ; and in order to show how the particle of light is affected by two refractions, it is arranged so as to enter the curved part of the medium.

Let A G B, fig. 3, be an hemispheric lens, the centre of which is D ; let G D N represent the axis of the lens ; a particle of light, G, moving in the direction of the axis, will touch the lens in a point coincident with its line of direction : hence no deflection can take place ; the particle of light, E, determined in the direction, E F, parallel to C G, the point of contact will be *v*; in that point it meets with less resistance than in any other point, consequently the deflection will be towards the perpendicular, which in this case is D F O, instead of proceeding in the direction F P; it will describe the line F T; and D T : D P = E O, as the sine of the angle of refraction is to that of incidence.

When the particle of light is arrived at the other side, A B, I is the point of contact; at that point, passing out of glass into air, where it is more resisted, the deviation will be from the perpendicular M F, and it will describe the line T N, and where it cuts the axis at N will be the focus. When the curve is uniform, making the allowance for aberration, which from figure is but trifling, all particles of light entering into that medium, the law of resistance

must be the same, and after being determined through the medium must all tend to one and the same point.

Let A B C D, fig. 4, represent a plano concave lens, whose radius of curvature is E G; E G Q will be the axis of the lens; any particle of light proceeding in this axis will, as before, continue unchanged as to its direction; but a particle of light, F L, will touch the curve in the point v, and entering into a less resisting medium will be deflected towards the perpendicular T E, and will move in the direction z x, so at the side, C D, the particle enters into a more resisting medium at the point S, the deflection from the perpendicular S P will be increased, and it will move in the direction x N, constituting a diverging ray.

Newton has demonstrated that the velocity of light through different media is in the ratio of their refractive powers; consequently the resistance must be in the same proportion diminished: all transparent bodies with respect to light may be compared to conducting bodies with respect to electricity; they are conductors of light; and the first portion of light transmitted by any transparent medium is that portion which was previously disseminated through the substance, impelled by the light acting upon the opposite side: so the spark we receive from the prime conductor of an electrical machine is the electricity which previously existed in the conducting body, impelled by the superinduced quantity, or more familiarly may be compared to a tube of water, when more water is determined to one end of the tube, the first portion which passes through the other orifice is that water which the tube previously contained. Then the conducting powers of bodies for heat, for electricity, or for light, are in nowise regulated by density: relative to light, we know it is regulated by the refractive power, and this power is greater in those substances which are inflammable, and which are hence supposed to contain the largest proportion of light; and hence probably some optical effects arise from that portion of light which enters as a constituent part into the composition of combustible bodies.

We hence may easily explain why semipellucid bodies, such as paper, are rendered transparent by being charged with substances, such as oil. By the interposition of a transparent medium a continuum of light is produced.

Dr. Priestley having observed that a particle of water floating over a vegetable leaf, not only preserves a globular form, but also at its inferior portion exhibits a black spot, he hence deduced from the 17th observation of the second book of Newton's Optics of " Observationes circa Colores Corporum tenuium pellucidorum," that the particle of water is not in contact with the substance of the leaf, but preserved hovering at a certain distance by a repulsive power. When the leaf is examined by a lens of $\frac{1}{4}$ of an inch focus, the explanation is easy, the particle of water is suspended by the hairs of the plant: if you break down the fibrillæ by your finger, no such appearances of the water will take place.

The greater the difference of resistance in different media, at a less angle will the light be reflected : thus out of water into air, where the refraction is nearly as three to four, it will be reflected at an angle of 48° 35° ; out of glass into air, where the refraction is to incidence as 20 to 31, light will be reflected at an angle of 40° 20°.

ARTICLE IV.

Observations on the Heat of Springs, and on Vegetation, in order to determine the Temperature of the Earth and the Climate of Sweden. By George Wahlenberg, M.D. Fellow of the Royal Academy of Sciences of Stockholm.*

DR. WAHLENBERG, who spent the last winter in Berlin, and is at present travelling through Switzerland, prepared the following treatise for the Annalen der Physik. It is scattered through the memoirs of the Academy of Sciences of Stockholm for the last three years, and deserves to be exhibited in one connected view. Dr. Wahlenberg has treated of the temperature of the earth in the north of Sweden with care and skill; not as a mere object of curiosity, but as a physical phenomenon, which constitutes a part of the physical constitution of the earth. I think I may flatter myself that my travels in the north have induced him to undertake these investigations. Mr. Von Humboldt's views, in his Geography of Plants, respecting the *southern line* of plants, have been followed out here with so much knowledge and exactness, that the results, I think, ought to find a place in the Annalen der Physik. Plants are now the objects by means of which accurate meteorological conclusions are to be drawn ; and philosophers may in a few weeks acquire as much knowledge of plants as is necessary for these kinds of investigation: and in how different a light does meteorology, degraded as it is by philosophical quackery, appear, when we draw our conclusions from so easy, but yet so accurate, a medium. Of how much more importance would it be for the philosophers of Leipzic to investigate their climate by the distribution of plants; as, for example, to ascertain whether the *chrysocoma lynostis* and the *onosma echioides* would spread in it, than to employ themselves in determining whether the conjunction of Jupiter and Venus would throw the interior organic parts into mighty labour in order to bring forth a mouse? Wahlenberg's experiments, how from springs, which are so very variable in their temperature, the mean tempera-

ture of the earth may be determined, by examining them at their origin, and the many interesting consequences which he has drawn from a collection of such temperatures, are decisive proofs of what may be done with a thermometer alone, without the assistance of any other instrument. LEOPOLD VON BUCH.

I.

Observations on the Heat of Springs, and the Temperature of the Earth, at Upsala, and at the Yngen Lake, in Wermeland. By George Wahlenberg.[*]

That the climate can be determined with the greatest precision by means of the plants which vegetate in any country, and that observations on the distribution of plants in different countries serve excellently to distinguish the climates of these countries from each other, scarcely require any proof. These observations on vegetation, indeed, do not furnish us with particular facts applicable to the whole surface of the earth, because it often happens that the same plants are not found in similar, but far distant, latitudes.

The temperature of the earth constitutes a much simpler measure, which is more easily determined, and is most intimately connected with the vegetation of plants. It constitutes, as it were, the mean between the peculiar temperature at which plants grow, and that of the atmosphere which surrounds them. Such determinations of the temperature of the earth furnish grand points in a chart of the climate of the country; while from the relations that plants bear to each other, the intervals between these points might be filled up. In order to satisfy myself fully of the truth of these observations, I have endeavoured to determine the temperature of the earth in the two places with the vegetables of which I am best acquainted, namely, at Upsala and at the Yngen lake, in the parish of Kroppa, which is under the mining jurisdiction of Philipstadt, in Wermeland.

The greatest spring at Upsala, in respect of the quantity of water which it contains, is the *Brennerei* (*distillery*) spring, or as it is also called, the *Sandviks* (*sandy*) spring. It forms at once a rivulet which is always capable, even at its source, of driving a small mill I have made the following observations on this spring. Its temperature was—

1808.	Temperature	1809.	Temperature.
30 March	43·9°	30 January	44·06°
10 May	44·06	28 February	44·24
6 June	43·9	30 March	44·06
25 Ditto	44·06	27 April	44·06
17 July	44·06	15 August	44·24
14 August	44·24	16 September	44·24
11 September	44·24	14 October	43·9

[*] Kongl. Vetensk. Acad. Nya Handl. 1809, p. 205.

	1808.	Temperature.
16	October	44·06°
13	November	44·06
14	December	44·24

These, and all the following observations, were made with the same thermometer, each degree of which was 0·0848 English inch in length.* Hence the tenth part of a degree could be easily observed on it. I thought it most accurate to graduate this thermometer myself; and to determine its highest fundamental point by means of the heat of the body. When we put the bulb of the thermometer under the tongue after seven or eight hours' sleep in the morning, and shut the mouth ; if we observe the height of the mercury by means of a concave mirror, we shall find that after an interval of a quarter of an hour it will stand at 98·366°. This experiment was frequently repeated, and the height of the thermometer never varied more than one or two fifths of a degree. If it be made in the same way about an hour after dinner, the mercury will stand at 99·32°, and this is the highest degree which it reaches. In the evening, after a little rest, it rises only to 97·52° ; and this is the lowest point. However, by means of warm food, the thermometer may be raised for an instant as high as 100·22° ; and by means of cold food it may be prevented for a short time from rising higher than 97·16°. After I had graduated my thermometer in this manner, I found it to agree very well with a thermometer of Dollond, with which Baron Hermelin had presented me in the year 1807, for my expedition to Lapland. I fixed the tube to a scale of hard fir, and to be able to determine correctly the height of the mercury, a slip of waxed paper, graduated on both sides, was pasted over one half of the tube ; and below it was a black mark which could be moved, and placed over against the place at which the mercury stood.†

I surrounded the bulb, which is 0·4712 English inch in diameter, with a triple fold of cloth, and left it in that state for an hour in the spring : as I had previously ascertained that it acquired the temperature of the liquid in which it was placed in three quarters of an hour.

From these experiments we may conclude that the temperature of the *Brennerei* spring at Upsala, during a year and a half, was constantly 44·06°, and therefore that this is the true temperature of the earth at Upsala.

Two other springs, well suited for such observations, on account of their strength and position, gave the following results :—

* The thermometer was of Celsius; so that each degree was equal to 1·8 degree of Fahrenheit.—T.

† More recently, in the year 1810, Dr. Wahlenberg attached an ivory scale to his thermometer ; hence in his later observations there is a difference of one or two tenths of a degree from his former ones.—*Von Buch.*

At Eric's spring, by the mill. $\Big\{$ 17 September ....44·24°
In 1809 .... $\Big\{$ 4 October ......44·06
                                  $\Big\{$ 17 Ditto ....44·06

Priest's well, by Haga. In $\Big\{$ 16 September ....44·24
1809 .... $\Big\{$ 10 October ......44·06

As these observations were made during a year, the mean temperature of which differed considerably from that of the springs; the heat of these springs seems to be constant; and the agreement of their temperature with each other cannot be considered as accidental.

At the south end of the lake *Yngen,* in the parish of Kroppa, by the town of Philipstadt, almost under the same latitude as Upsala, but more elevated above the level of the sea, I found in the year 1809 the heat of

The spring of *Ribbhögdlottn,* $\Big\{$ 2 July ....40·82°
near little *Angtjärn* ...... $\Big\{$ 10 Ditto ....41·18
                                 $\Big\{$ 5 August ......44·42

The spring at *Nykroppahog-* $\Big\{$ 5 July ....42·62
den, on the south-east of the $\Big\{$ 9 Ditto ....41·00
great furnace .... $\Big\{$ 6 August ......42·80

These last springs have diminished so much of late, that no motion whatever can be perceived in their bottom. It is obvious that the drought, which has diminished these springs, must have somewhat increased their temperature.

The *Ohs* spring (*Ohskälla*), somewhat less than an English mile north from Herbult, and near a small lake called *Get Plohgan,* on the west side of the river *Hult,* in Vermeland, is a strong spring, which rises from a sandy bottom about a fathom deep. Its temperature was, in 1809—

25 July ....41·18°
6 August ....41·00
7 Ditto..........................41·00
21 September ....41·00

The last observation was made after my departure by Gustavus Wahlenberg, with a thermometer graduated like mine. The temperature of this spring, which was determined during a year in which the heat of more scanty springs was variable, may be considered as constant. Hence I believe that we may, without sensible error, reckon the temperature of the earth at the lake *Yngen* as 41°.

Were we acquainted with the rate at which the temperature of small and large springs varies, a few slight observations on the temperature of such springs would make us acquainted with that of the earth. Experiment has shown us that April and October com-

monly exhibit the mean temperature of the year; and that July is the hottest, and January the coldest, month. It would seem that these changes show themselves in small springs 1½ month later, and in great springs 1¾ month later: but the heat of the atmosphere, when once it has reached the medium, changes most rapidly; while at the extremes it remains some time without sensible alteration. On that account the temperature of springs changes rapidly towards the extremes, and remains long unaltered towards the medium of the temperature of the air; and its increase and diminution follows a very regular rate, on which the most rapid alterations in the heat of the atmosphere have little or no effect. The following observations on a scanty and a more copious spring will show the nature of these alterations more clearly.

Källsprohng, at *Lassbybackar.* In 1809.		*Professor spring at Haga.* In 1809.	
30 May	44·06°	11 September	48·74°
18 August	54·68	16 Ditto	48·56
2 September	55·40	1 October	48·02
11 Ditto	52·93	10 Ditto	47·66
18 Ditto	52·16	17 Ditto	47·12
1 October	49·10		
9 Ditto	46·94		
10 Ditto	46·76*		
17 Ditto	45·32		

It is at least clear from this table that the springs become warmer till towards the beginning of October. Now as the *Ohs* spring, near *Herhult,* remained unaltered on the 25th July, the 7th August, and the 21st September, I think we must conclude from hence that the temperature of this spring remains always the same; and that therefore the temperature of the earth at the lake *Yngen* sea is 41°, or 3·06° less than that of the earth at Upsala. This lake lies in one of the highest spots of Wermeland. From several levelling observations we found its height above the level of the sea was 549 English feet. This gives us 179½ English feet for an alteration of one degree of Fahrenheit in the mean temperature; yet the usual allowance in the Swedish climate has been an alteration of 1° of temperature for 260 English feet of elevation. Should we choose to reckon the ten minutes that the south end of the lake *Yngen* lies farther south than Upsala, as equivalent to an increase of temperature amounting to 0·216 of a degree; still the temperature of the earth at the lake Yngen would be 1·188° lower than at Upsala, which is a third more than we were warranted to suppose;

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* These observations followed each other so rapidly in order to observe whether the heavy fall of snow between the 8th and 10th of October had produced any effect on the springs. The effect was not remarkable. How regularly the temperature of these springs varies will be easily perceived by making the degrees and the time the ordinates and abscissæ of a curve.—*Wahlenberg.*

a proof that besides the latitude and the height above the level of the sea, there is still another circumstance, namely, the neighbourhood of the mountains, to be taken into account.

The melting of the snow shows clearly the great difference between the temperature of Upsala and the lake *Yngen*; for the seasons do not vary more from each other in different climates than they do in these two places in the early part of the year, and especially in the time that the ice breaks up. This happens at Upsala between the 14th and 23d of April; but at the lake Yngen, never sooner than between the 12th and 20th of May.*

The variations in the vegetation correspond with the differences observed in the temperature of these two places. Various plants reach their northern limit before they arrive at the lake Yngen, which, notwithstanding, continue to vegetate around Upsala. There are likewise various northern plants which reach their southern limit at Upsala that still continue to flourish by the side of the lake Yngen. These plants cover broader or narrower zones of the earth's surface, which may be pointed out by lines, both on their north and south side. On the one side, the too high temperature prevents the plants from growing; on the other, the too low temperature. Now these limits, as well of northern plants towards the south, as of southern plants towards the north, mark with precision the temperature of the earth at the respective places.

Upsala.

The following are the *southern plants* which reach their northern limit not far from Upsala, and which are quite unknown in the country surrounding the lake *Yngen.*

The *oak* in the neighbourhood of Upsala requires the shelter of the acclivities of mountains, and scarcely passes the river Dal, where the temperature is 43·34°. In Vermeland it is found only in the neighbourhood of the lake Venner, and about Kummelö. The elm (*ulmus campestris*) can bear a somewhat more severe climate; but it will not grow at the lake *Yngen.* The case is the same with the following plants : *Cratægus oxyacantha, prunus spinosa, rosa canina, rhamnus catharticus, salix fragilis, mespilus cotoneaster, rubus cæsius, ononis arvensis* (uncommon also about Upsala), *primula veris, pulmonaria officinalis, stratiotes hydrocharis, typha angustifolia, anemone pulsatilla, centaurea scabiosa, spiræa filupendula, butomus, lychnis viscaria, rumex crispus, conwallaria polygonatum, ornithogallum luteum, asclepias vincetoxicum, malva rotundifolia β thalictrum flavum, agrimonia, convolvulus arvensis, tordylium anthriscus, hottonia, anchusa officinilis, cynosurus cæruleus, phalaris phleoides, scirpus sylvaticus* and *caricinus, carex muricata* and *flacca, psyllophora.*

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* At Abo it happens on April 16 or 17; at Umeo, on May 7; at Uleo, on May 8; at Sodankyla, lat. 67° 23′, on May 18; at Utsjocki, Tana-elf, on May 20.—*Von Buch.*

The *northern plants,* which cease to vegetate at Upsala, or are very seldom seen there, but which are very common at the lake Yngen, and which reach their southernmost limit at Upsala, are the following:—

There covers the zone.	Temperature of the earth.
Rubus chamæmorus	From 43·7° to 35·8°
Linnæa borealis	From 44·06 to 35·6
Polygonum viviparum	From 44·06 to 33·8
Calla palustris (a smaller zone)	From 44·42 to 37·4
Trientalis Europæa	From 44·06 to 34·7

Andromeda polifolia, scheuchzeria, carex leucoglochin.

The Lake Yngen.

The *southern plants* which reach their northern limit at the lake Yngen, which are seen there but seldom, though they are common about Upsala, are the following: —

The *ash* (*fraxinus excelsior*), and the *common guelder rose* (*viburnum opula*), do not grow here in exposed situations, but require the peculiar shelter of very narrow valleys. The *maple* (*acer platanoides*), and the *lime-tree,* are to be found only on some peculiar acclivities which are very favourably exposed to the powerful action of the sun's rays. There likewise, and only there, are to be seen two miserable *hazle* bushes. *Saxifraga granulata, hyoscyamus,* and *leonurus cardiaca,* reach quite as far as *Storforsbruck.* There occur likewise, *rosa villosa, salix fusca, galium verum* (*verbascum thapsus, geranium sanguineum,* and *orobus vernus,* only in one place, by *Pehrsberg; phellandrium aquaticum,* only in *Nykytta-elf,*) *antirrhinum linaria, geranium robertianum, linum catharcticum, ophrys ovata,* &c. *Alnus glutinosa* is likewise found in abundance. It disappears when the temperature of the earth sinks to 39·2°. Attempts have been made to cultivate the horse-chesnut (*æsculus hippocastanum*) near *Gammalkroppa ;* but as the branches are annually frozen, the stem always at last dies before it has reached the height of a fathom. Equally unsuccessful have the attempts been to raise fruit-trees in this place.

The *northern plants* which reach their southern limit at the lake *Yngen,* and are only first seen considerably to the north of Upsala, are—

	Temperature of the earth necessary for their growth.
Alnus incana	From 41° to 36·5°
Betula nana	From 41 to 33·8
Carex globularis, a peculiar small zone	From 41 to 37·4

Carex limosa, irrigua, livida, and *scirpus cæspitosus.*

We might easily believe that plants penetrate by their roots to so small a depth in the ground as to be able to experience, in a greater

or smaller degree, the permanent temperature of the earth: but it is surprising to how small a depth in the earth the great alterations in the temperature of the air are able to penetrate. Indeed, they are almost entirely confined to the surface. The small spring of *Lassbybackar* appears, from an examination of the spot, scarcely to reach a foot below the bottom. Larger springs may go some feet deeper. However, from their temperature the influence of the constant temperature of the earth is not to be learned. Several trees and perennial plants shoot their roots much deeper, and therefore reach a still more uniform temperature. The roots of the palms, for example, penetrate several fathoms perpendicularly downwards into the soil: and this may probably be the reason why the palm, when compared with so many other families of plants, is confined to so narrow a zone; for it is only in very hot climates that the roots at such a depth can meet with the temperature requisite for the vegetation of palms. This is probably, likewise, the reason why it is so much easier for us to raise in our gardens annuals than perennial plants. The latter, in consequence of the greater depth of their roots, feel more the effects of our cold climate than the former.

The same species of trees cease to vegetate on the mountains in warmer countries at a much higher temperature: so that their distance from the snow line is twice as great as with us; but similar herbs in both places rise to the same distance from the snow line. On the southern mountains the difference of the various seasons of the year is very small: the heat of the summer does not produce such an effect upon the soil as it does farther north: hence the trees which, sinking deeper into the earth than the herbs, require a higher temperature in the soil, cannot approach so near the snow line as they can in higher latitudes. Probably this greater difference in the seasons of the year is the reason why the temperature of the earth in the north is higher than the mean temperature of the atmosphere.* It would appear from the observations of Von Humboldt that the contrary is the case in warm climates; for the sun's heat shows its intensity and duration upon the dry soil, while the winter's cold, in consequence of the coat of snow with which the earth is covered, acts but feebly upon the soil.†

The influence of the sun's heat is generally very different upon wet and dry soils. In the latter it penetrates to a considerable depth; but not in the former. Moving water in lakes and rivers very readily acquires the temperature of the atmosphere; but it prevents that temperature from penetrating deeper, in consequence

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* Mean temperature of the atmosphere at Upsala, from 80 years' observations ....42·03°
Temperature of the earth at Upsala ....43·70
                                                                        Von Buch.

† During winter, likewise, the greatest vehicle of the equalization of temperature is wanting, namely, the veins and drops of water spreading over the whole soil with the temperature of the surface.—*Von Buch.*

of the absence of those conductors of heat which are to be found in mosses, bogs, and morasses. Hence the plants which cover dry declivities on high mountains, in warmer places appear only in deep morasses, because it is only in such places that they find the low temperature which they require. This is the case with *betula nana*, *andromeda polifolia*, *narthecium boreale*. In Switzerland these plants do not rise to the higher mountains. Perhaps the morass of *Skatelöf*, in Smoland, in which we so unexpectedly meet with *betula nana*, is cooled by means of springs, which oppose here the influence of the sun's rays more than in other places. To this want of conductors of heat, either immediately, or through the failing of the circulation of water, and the consequent low temperature of the place, is to be ascribed the great art and trouble requisite to render the northern provinces of Sweden equally fruitful as temperate climates. In Lapland the driest places only are fit for the purposes of agriculture.

II.

On the Temperature of the Springs at Upsala in the Year 1810. By George Wahlenberg.

We seldom find springs which undergo a great alteration in their temperature, and yet in dry and wet years furnish an equal quantity of water. Such, however, is the case with the spring at *Lassbybackar*. There are two veins of water, the southernmost of which rises immediately out of the sand. I found the changes in its temperature as follows :—

Spring at Lassbybackar, near Upsala.

1809—	1 November	44·96°		1810—	1 May.....	34·16°
	5	....	43·88		6	....	34·16
	11	....	42·62		10	....	34·16
	20	....	40·46		13	....	34·34
	29	....	38·81		18	....	34·16
	5 December	38·48		24	....	35·06
	13	....	38·48		25	....	35·42
	22	....	37·40		28	....	36·50
	24	....	37·58		30	....	37·76
1810—	4 January..	37·40		3 June	39·20
	19	....	36·68		6	....	41·18
	27	....	36·50		10	....	43·34
	6 February.	36·14		13	....	43·70
	8	....	36·05		17	....	45·86
	12	....	36·05		25 August ..	54·50
	22	....	35·42		1 September	53·96
	4 March...	35·06		25	....	51·08
	13	....	34·70		2 October..	50·00
	25 April....	34·16		12	....	48·02
	28	....	34·16		25	....	44·24

Mean of the whole year .....41·72°

I made a delineation of these observations upon paper as the abscissæ and ordinates of a curve, and determined from the curve the mean temperature of each month. From these means, the medium for the whole year, as stated above, was deduced. It agrees pretty nearly with the mean temperature of the atmosphere; but stands accidentally almost two degrees lower than the constant temperature of the springs at Upsala. As during the months of April and May this spring was remarkably colder than the air, so its higher temperature was equally conspicuous during the first winter months; and the final result shows that even a spring, which changes in its temperature in so extraordinary a manner as this, still deviates very little from the mean temperature of the air.

That this difference from the temperature of the air increases in proportion as the temperature of the springs becomes more constant, may be learned from the following observations.

The *Professor spring*, at *Haga*, varies in its temperature only half as much as the preceding spring. I found it—

1809— 5 November 45·50°	1810—18 April .... 38·57°
11 .... 45·14	1 May .... 38·30
20 .... 44·14	6 .... 38·48
29 .... 43·88	10 .... 38·48
6 December 43·16	18 .... 38·48
25 .... 41·54	25 .... 39·02
1810— 4 January.. 41·54	30 .... 39·38
17 .... 41·54	6 June .... 39·92
27 .... 41·18	10 .... 40·37
8 February. 40·28	17 .... 40·82
12 .... 40·46	25 August .. 46·04
22 .... 40·46	1 September 46·04
6 March... 40·10	25 .... 46·76
16 .... 39·74	2 October.. 46·76
28 .... 39·29	12 .... 46·49
7 April .... 38·48	25 .... 45·86

Mean of the whole year ..... 42·26°

The mean temperature of this spring is higher than that of the preceding, almost in the same proportion as its differences of temperature are less. In both springs the rate of the variations is nearly similar.

Another spring, situated in the same district as the *Professor spring*, and lying north-west from it, is still more constant in its temperature. I found its heat as follows :—

1809—13 December 43·88°	1810— 6 May .... 42·08°
25 .... 43·88	30 .... 42·08
1810—27 January.. 43·52	27 August .. 43·16
7 February . 43·34	25 September 43·88
25 April .... 42·08	

Mean ....42·98°

Here also the mean has become higher, in proportion as the scale of variations has diminished. In other respects this last spring differs from the two preceding in the greater length of time during which it remains at its highest temperature, and likewise in the long period during which its lowest temperature continues. The temperature curves of all these springs show that the deeper springs have a higher mean temperature ; and it follows, as a consequence, that the heat of summer penetrates farther into the earth than the cold of winter.

These observations show that all springs are coldest at the end of spring, and hottest, on the contrary, in harvest : but the variable springs reach their highest temperature about the end of August, a short time after the temperature of the air has arrived at its maximum : but the constant springs reach their highest temperature in September. From this it follows that the nature of such springs may be determined from a few observations made at these seasons of the year. The following observations, though made only a few times during the course of the year, make us acquainted with every thing requisite to be known respecting the springs examined.

There is a spring at *Gnesta-Windmühle,* on the road to *Fundbo,* which rises directly out of a sand hillock, and in such abundance that it immediately forms a rivulet. Next to the *Sandviks* spring, it may be considered as the greatest in all that tract of country. This spring gave the following temperatures :—

> *Gnesta quarn källa,* 1810—13 May ....42·53°
> 26 August ....42·80
> 26 September ......43·16
> 27 October ....43·43

The mean temperature of this nearly permanent spring appears to agree pretty well with that of the preceding.

A spring at the rivulet below *Norbygard,* in the parish of *Bond-kyrka,* in a clay soil :—

1809—24 December	42·98°	1810— 1 May	....	42·80
1810—24 January..	42·98	25	....	42·80
27	42·98	10 June	....	42·80
22 February.	42·80	25 August	..	42·98
31 March ..	42·80	27 September		43·34

Not far from this, and near *Norbyland,* well known to the botanists of Upsala, lies the spring of *Norbyland.*

> 1810—25 May ....42·80°
> 1 September ....43·34
> 27 ....43·88

At Mysbygard, in the parish of *Danmark,* there is a spring which rises from a flat bottom with such violence that it forces up the gravel half a foot high in the water. It is one of the most constant springs. It gave—

1810—13 May .... 43·16°   1810—26 September 43·34°
    26 August... 43·16   27 October .. 43·34

This is the only constant spring at Upsala, whose temperature is lower than the mean temperature of the earth for Upsala. The other springs agree better with the results formerly obtained.

Staby spring, in the parish of Naes, spouts up strongly from the bottom of a high sand hillock :—

    1810—14 May ....43·79°
         27 August ....43·70

Priest's well, at Haga, mentioned before, rises at the foot of a hillock ten fathoms high. The stability of its temperature is in some measure regulated by the *Quarnber* rivulet, which runs by the foot of the hillock. I found it as follows :—

1809— 5 November 44·06°   1810—30 May..... 43·70°
      29 .... 43·79   25 August .. 43·88
1810—25 May..... 43·70   25 September 43·88

Mill spring (*Quernkälla*), at Upsala. It is surrounded by a small heap of stones:—

1809—12 November 43·97°   1810—29 March ... 43·79°
      27 .... 43·88   13 May..... 43·70
1810—17 January.. 43·79   31 August .. 44·06
    24 February . 43·79   30 October.. 43·88

Brännerey, or *Sadviks*, spring, mentioned before:—

1809—21 November 43·88°   1810—26 April .... 43·52°
      26 .... 43·88   11 May..... 43·61
    22 December 43·79   30 August .. 43·70
1810—28 January.. 43·88   28 September 43·88
    23 February . 43·79   30 October.. 43·88
    29 March ... 43·79

I conceive that at a greater depth the *Myrbygard* and *Sandvik* springs would agree in their temperature, and that the difference between them is owing to the effect of the current immediately under the surface. In order to examine this effect, I sought for springs rising directly out of the earth at the bottom of lakes or rivers. I found two springs of this kind in the neighbourhood of Upsala.

A little below the tile manufactory, one-eighth of a Swedish mile from the town, a spring rises with a strong current in a rivulet. Its temperature, on the 23d of September, 1810, was 43·70°.

The remarkable spring of *Strömsholm* rises from the bottom of the lake *Ladugard*, in a creek in the great stream, and not far from *Camrersgard*. Some hundred veins, at the depth of 12 feet below

the surface, form a great current at least two fathoms in circumference. On the 23d of September, 1810, the thermometer placed in it stood at 44·06°.

As experience has now shown that the colder springs flow near the surface of the earth, I think myself entitled to infer from my observations, continued during several years, that 43·70° is very nearly the true mean temperature of the earth at Upsala.

(*To be continued.*)

ARTICLE V.

New Observations on the Composition of Alcohol and Sulphuric Ether. Presented to the French Institute by M. Theodore de Saussure.[*]

1. THE experiments which I published some years ago on alcohol and sulphuric ether[†] were sufficient to show that ether contains more carbon and hydrogen than alcohol; but they were far from showing the true proportion of the elements of these two liquids. I took care to announce that these first results were mere approximations, and contained only the rudiments of a set of experiments which would be resumed and completed.

A great simplification of the means which I formerly employed for the analysis of ether, some improvements in the eudiometrical processes, and some corrections in the data from which these analyses are calculated, enable me at present to lay before the public much more correct results.

The methods of analysing vegetable substances have not yet reached that precision which is exhibited in the analysis of different mineral substances. Such extreme accuracy would in all probability be useless if applied to bodies like alcohol and ether, the degree of purity of which is not yet ascertained: but we shall find that these two liquids have a property which determines with precision the manner of their composition.

The data which serve as the basis of my calculations are the specific gravities of the gases as determined by Biot and Arago, the weight of the cubic decimetre (61·028 cubic inches) of atmospheric air being 1·293 gramme (19·969 grains troy), at the temperature of 32°, when the barometer stands at 29·922 inches, and when the hygrometer marks extreme dryness.[‡] I admit that in these circumstances the cubic decimetre of carbonic acid gas contains 0·5378 gramme (8·306 grains troy) of carbon, and that 100 parts by weight

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[*] Translated from the Bibliotheque Britannique, vol. liv. No. iv. Dec. 1813.
[†] Jour. de Phys. t. lxiv.
[‡] This would make the weight of 100 cubic inches of dry air, at the temperature of 60°, 30·904 grains troy, which I believe about 0·404 grain too high.—T.

of this gas are composed of 72·63 oxygen and 27·37 carbon: 100 parts of water by weight contain 88·3 of oxygen and 11·7 of hydrogen, the volumes of these gases being to each other as two to one.

SECTION FIRST.

Analysis of Alcohol.

2. *Decomposition of Alcohol by means of a red-hot Porcelain Tube.*

The analysis of alcohol by burning it in a lamp does not give a correct result, because a portion of the liquid is volatilized without undergoing combustion. When the vapour of alcohol, mixed with oxygen and hydrogen gas, is detonated in a eudiometer over mercury at the temperature of the air, the combustion is complete; but the operation is very complicated, and the quantity wrought on is too small to entitle the result to any confidence. When alcohol is distilled very slowly through a red-hot porcelain tube, it is almost wholly converted into water and a gas, which may be analysed without any difficulty. I have adopted this method, which is the most exact of all those with which I am acquainted.

The alcohol which I employed was prepared by mixing spirit of wine with its own volume of water, and subjecting the mixture to successive slow distillations, preserving only the portions which passed over first in each. I obtained it of the specific gravity 0·8302, at the temperature of 62·8° Fahrenheit. This result indicates, according to the table of Richter,* a liquid composed of 13·8 water and 86·2 absolute alcohol of this author, which I shall denote by the name of alcohol of Richter. Its specific gravity is 0·792, at the temperature of 68° Fahrenheit.

The result of my analysis has been reduced by calculation to what would have been obtained with the alcohol of Richter, because that alcohol serves as the basis of the most copious and accurate table that we have respecting the specific gravities of mixtures of water and alcohol; but this base is not pure alcohol, as Mr. Hutton obtained an alcohol of the specific gravity 0·784, at the temperature of 66°, which did not appear to be pure alcohol.†

I employed an alcohol mixed with a good deal of water, and rectified without the application of any saline substance, to guard against the objections of those authors who are of opinion that alcohol undergoes a chemical change by being concentrated according to the process of Richter, by means of muriate of lime. This alteration, which may be a purification (since in depriving it of water we separate those substances which are only soluble in water), is indicated by slight alterations in its flavour and smell, but occasions no perceptible alteration in the analysis. When the two

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* The correction which that table requires was made from Gilpin's tables.
† Experiments on the congelation of alcohol by Mr. Hutton, Bibl. Brit. vol. liii.

liquors contain the same quantity of water, we observe between them, either with respect to changes in their specific gravity by farther additions of water, or with respect to their other properties, differences too slight to be exactly determined, or to have any influence in the experiments about which I am occupied.

I distilled slowly over a water bath, through a tube of porcelain heated to redness, and inclined a little, 81·37 grammes (1256·7 grains) of the alcoholic liquor, which, as indicated by its specific gravity, contained 70·14 grammes (1083·3 grains) of the alcohol of Richter, and 11·23 grammes (173·4 grains) of water. The products of this operation passed into a narrow glass tube, a metre and a half (59·056 inches) long, surrounded with ice, and answering the purpose of a worm. It communicated with a small globular vessel placed to receive the liquid products, and to transmit the gas to the water trough. This distillation continued 14 hours, and furnished,

1. In the porcelain tube 0·05 gramme (0·77 grain) of charcoal.

2. Chiefly in the glass tube 0·41 gramme (6⅓ grains) of a mixture of volatile crystals in thin plates, and an essential oil, brown coloured, liquid, and having a strong odour of benzoin and empyreumatic. This product, whatever be its composition, is too small in quantity to be of much consequence in this analysis. The experiments which I have made on some essential oils induce me to consider them as composed of 0·7 carbon, 0·2 oxygen, 0·1 hydrogen. I have applied this estimate to the oil obtained by the decomposition of alcohol.

3. I collected in the globular vessel, and in other parts of the apparatus, 17·24 grammes (266·3 grains) of water, destitute of colour, and slightly impregnated with alcohol. Its specific gravity, at the temperature of 68°, was 0·9942. This indicates a mixture of 16·59 grammes (256·2 grains) of water, and 0·65 gramme (10·1 grains) of the alcohol of Richter. This reduces the 70·14 grammes of alcohol operated on to 69·49 grammes (1073·2 grains). This water reddened vegetable blues, and had the taste and smell of vinegar. It formed acetate of potash with that alkali; but the weight of the salt amounted only to five centigrammes (0·77 grain). This water was likewise rendered slightly muddy by nitrate of silver, and gave out vapours scarcely sensible at the approach of muriatic acid; but this acid did not produce a ponderable quantity of muriatic acid. In consequence of the very small proportions in which these substances were present, I may consider the liquid as a mere mixture of water and alcohol, in the proportions indicated by the specific gravity.

4. The oxycarbureted hydrogen gas resulting from the decomposition of the alcohol, at the temperature of 52¼°, when the barometer stood at 28·375 inches, and when saturated with moisture, occupied the bulk of 87·07 litres (5313·7 cubic inches), which weighed 60·25 grammes (930·5 grains), taking the mean of five

different products. It did not contain a notable quantity of carbonic acid, at least the proportion of this gas did not exceed $\frac{1}{200}$th, and I neglected it.

The volume of oxycarbureted hydrogen gas reduced to extreme dryness, at the temperature of 32°, and when the barometer stands at 29·922 inches, would be 77·924 litres (4744·7 cubic inches). It now weighed only 59·069 grammes (912·3 grains) in consequence of the subtraction of 1·181 gramme (18·2 grains) of water, which ought to be added to the 16·59 grammes (256·2 grains) of water obtained as the third product.

3. *Analysis of the Oxycarbureted Hydrogen Gas obtained from the Decomposition of Alcohol in a red-hot Porcelain Tube.*

The cubic decimetre (61·028 cubic inches), at the temperature of 32°, and when the barometer stands at 29·922 inches, weighed at a medium 0·75804 gramme (11·707 grains). This weight during the course of the experiment underwent alterations which did not exceed a centigramme (0·1544 grain). They did not depend, as I had at first suspected, on the accumulation of charcoal in the tube, but upon the degree of heat to which the gas had been exposed. When the distillation became more rapid, and the porcelain tube less hot, more oil was obtained, and a heavier gas, containing a greater proportion of carbon. From a mean of all these products, it follows that 100 measures of the gas decomposed by detonation with 300 measures of oxygen gas over mercury,* consumed 121·95 measures of oxygen gas, forming at the same time 81·15 measures of carbonic acid gas. It contained no azotic gas.†

It results from these data that 100 parts by weight of this oxycarbureted hydrogen gas, supposing it perfectly dry, are composed of

Carbon	57·574
Oxygen	28·466
Hydrogen	13·960
	100·000

These products contain 10·189 parts of hydrogen, besides the elements of 32·237 parts of water.

The gas which I have analysed has this remarkable property : *the*

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* In my former experiments this gas, as well as that obtained from ether, was decomposed over mercury.

† Not to be deceived respecting the presence or absence of azotic gas in oxycarbureted hydrogen, we must analyse it before it has stood any length of time over water; because that liquid gives out to it the air which it contains. Alcohol does not contain a perceptible quantity of azote. I have found by experiments with the apparatus of Meusnier to collect the liquid formed when it burns in the open air, that the water formed by the combustion of alcohol and sulphuric ether contains in solution a sensible quantity of ammoniacal salts, but is notwithstanding almost pure : but I have found that pure hydrogen, and all hydrogenous substances, though free from azote, furnish in these circumstances either ammonia or nitric acid, or these two substances united, and that the azotic gas of the air disappears at the same time.

volume of oxygen which it requires for its complete combustion is to
the volume of carbonic acid gas produced as three to two.

The weight of hydrogen in excess above the elements of the
water is to the weight of the charcoal as 1 : 5·65.

Olefiant gas (called at present *percarbureted hydrogen*) contains
no oxygen : but with respect to the hydrogen and carbon which it
contains, it follows the same law as the oxycarbureted hydrogen,
when in this last gas we consider only the oxygen in excess above
the elements of water.*

The carbonic acid is to the oxygen gas consumed as two to three,
100 parts of this gas consume in volume 300 of oxygen, and form
200 of carbonic acid gas. The weight of the cubic decimetre
(61·028 inches) of olefiant gas being 1·2649 grammes (19·535
grains) at the temperature of 32°, and when the barometer stands
at 29·922 inches.† It follows from this that 100 parts by weight of
olefiant gas are composed of

Carbon .... 85·03
Hydrogen .... 14·97
—————
100·00 ‡

The hydrogen is to the carbon as 1 : 5·68; as is the case in the
oxycarbureted hydrogen just analysed, when we consider only the
hydrogen in excess above the elements of the water.

It is evident from the comparison of these two gases that the
oxycarbureted hydrogen from alcohol is merely olefiant gas and
water reduced to their elements; for if in the analysis of the oxy-
carbureted hydrogen we add to the 57·574 parts of carbon the
10·189 parts of hydrogen which are in excess above the elements
of the water, we have a compound of 67·763 parts which is con-
stituted like olefiant gas, while the 32·237 parts that remain are
constituted like water. Thus the oxycarbureted hydrogen obtained
from alcohol by a red-hot tube is represented by 100 olefiant gas
and 47·6 water; or, in other terms, by the principles of olefiant
gas, and about half their weight of water reduced to its elements.

4. *Application of the preceding Results to the Analysis of the
Alcohol of Richter.*

If we resume the products of the decomposition of alcohol by

* Olefiant gas is composed of an atom of hydrogen weighing 0·132, and an
atom of carbon weighing 0·751. Now 1 : 565 :: 0·132 : 0·746; so that the esti-
mate of Saussure does not deviate much from the truth.—T.

† Olefiant gas, according to the way in which it is prepared, may have a
specific gravity differing a little from that given here; but in that case 100 mea-
sures do not consume exactly 300 of oxygen, and form 200 of carbonic acid gas.
Hence I consider it as doubtful whether in such cases it be pure olefiant gas. The
employment of those numbers, which are within the limits of observations, is
justified by the consideration that gases combine in volumes which have a simple
ratio to each other.

‡ I consider this gas as composed of 85·05 carbon + 14·95 hydrogen, which
almost exactly agrees with the statement of Saussure.—T.

heat in close vessels, we find that 81·37 grammes (1256·7 grains) of weak alcohol, of the specific gravity 0·8307, furnished the following products :—

	Grammes.	English Grains.
Oxycarbureted hydrogen	59·069	912·30
Water	17·771	271·40
Oil	0·410	6·33
Charcoal	0·050	0·77
Alcohol of Richter	0·650	10·10
Loss	3·420	55·82
	81·370	1256·72

If the small loss be considered as aqueous alcohol not decomposed, the alcohol decomposed will be 77·95 grammes. If we reduce these results to what they would have been if we had employed the alcohol of Richter, all the above products remain as before, but the water is now reduced to 6·541 grammes, and the quantity of alcohol of Richter decomposed amounts to 66·07 grammes. When for these products we substitute their elements, 100 parts of alcohol of Richter are composed of

Carbon	51·98
Oxygen	34·32
Hydrogen	13·70
	100·00

We find in this result 9·15 parts of hydrogen in excess over and above 38·87 parts of water reduced to their elements.

This hydrogen in excess in the alcohol is to the carbon as 1 : 5·68, just as in olefiant gas and in the oxycarbureted hydrogen gas obtained by the decomposition of alcohol. It is easy to see that 100 parts of the alcohol of Richter are represented by the elements of 61·13 parts of olefiant gas and 38·87 of water.

To obtain from this analysis the volume of oxygen gas which a given weight of alcohol consumes in burning, and the volume of carbonic acid gas which it forms at the same time, it is sufficient to reduce by calculation the 51·98 parts of carbon into carbonic acid gas, and to consider that the volume of oxygen consumed must be equal to the volume of carbonic acid gas produced, and to half the volume of the hydrogen gas in excess. Thus a gramme (15·444 grains) of alcohol of Richter requires for its combustion 1449·84 cubic centimetres (8848·37 cubic inches) of oxygen gas, and forms at the same time 966·54 cubic centimetres (5898·77 cubic inches) of carbonic acid gas.

Now these two numbers are to each other as 2 : 3. Hence I conclude that alcohol is represented by the combination of water

6

and olefiant gas reduced to their elements.* The alcohol of Richter, the specific gravity of which is 0·792 at 68°, is composed of 100 parts by weight of olefiant gas and 63·58 of water. We are not acquainted with alcohol entirely free from water; but it is sufficient at present to have determined the manner according to which that liquor is composed, and to have nearly indicated the proportion of the elements of a mixture of pure alcohol and water of a determinate specific gravity.

SECTION SECOND.

Analysis of Sulphuric Ether.

6. *Decomposition of Ether by means of a red-hot Porcelain Tube.*

The ether which I employed for these experiments had been rectified by three successive operations :—1. It was distilled from a solution of potash till one half came over. 2. The product was washed with twice its weight of water. 3. It was then distilled from muriate of lime at the temperature of 91·5°, and only the third part of the liquor was collected for experiment. The ether thus obtained was of the specific gravity 0·7155, at the temperature of 68°. I distilled, at a temperature below that at which ether boils, 47 grammes (725·868 grains) of that liquid through a red-hot porcelain tube. This apparatus was similar to that described for the analysis of alcohol. The operation lasted nine hours. The products were—

1. In the porcelain tube a small quantity of charcoal, which weighed 0·12 gramme (1·85 grain).

2. An empyreumatic oil. It was brown, liquid, volatile, and soluble in alcohol. There was likewise a thick blackish substance, only soluble in ether, and analogous to pitch. There were likewise crystals in thin plates, less volatile than the brown oil, and similar to those which the alcohol produced when treated in the same way.† The mixture of these three substances weighed 0·4 gramme (5·178 grains).

3. There was disengaged 55·85 litres (3408·4 cubic inches) of gas at the temperature of 61¼°, and when the barometer stood at 0·7309 metre (28·776 inches), and saturated with moisture. The first portions, constituting a third of the whole, contained $\frac{1}{100}$ of their volume of carbonic acid gas ; the remainder contained none of that substance. These 55·85 litres weighed 43·245 grammes

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* The proportions found by Saussure make alcohol a compound of two integrant particles of olefiant gas and one of water ; or of two atoms carbon, three hydrogen, and one oxygen.—T.

† They are soluble in alcohol. They disappear in water at the end of some days. In oxygen gas they only burn at a red heat, and in the state of vapour. They are not decomposed by repeated volatilizations in this gas at a temperature below incandescence. Oil of rosemary and of oranges passed through a red-hot tube furnish them equally ; but I have always obtained them in too small a quantity to be able to determine exactly their nature and properties.

(667·88 grains) ; but as no water was disengaged, we may suppose that the gas, before it came to the pneumatic trough, was destitute of hygrometrical water. In that case it would only have weighed 42·36 grammes (654·21 grains). The gas carried along with it a yellow vapour, which appeared to consist of the liquid empyreu-matic oil mentioned in the second product. It was most abundant when the porcelain tube was least hot, and the distillation most rapid.

Forty-seven grammes (725·868 grains) of sulphuric ether then, when decomposed by the process here followed, produced

	Grammes.	Grains Troy.
Gas	42·36	654.21
Charcoal	0·12	1·85
Oil and pitch	3·40	5·178
Loss................	4·12	64·63
	47·00	725·868

This loss, which amounts to about nine per cent., and which seems owing to the volatilization of an oil not ascertained, is too great to enable us to draw accurate conclusions from my experi-ments on the constituents of ether : but we may form an approxi-mation from the analysis of the gas obtained, which amounts to about nine-tenths of the whole ether decomposed.

7. Analysis of the Oxycarbureted Hydrogen obtained by passing Ether through a red-hot Porcelain Tube.

The specific gravity of this gas (abstracting the small quantity of carbonic acid gas above mentioned) increased when the distillation was conducted with greater rapidity, and when the gas experienced less heat. When this gravity was at its maximum, the cubic deci-metre (61·028 cubic inches) of gas weighed 0·85808 grammes (13·252 grains), at the temperature of 32°, and when the baro-meter stood at 29·922 inches, and supposing it deprived of all moisture : 100 measures of it mixed with 300 measures of oxygen gas, when detonated over mercury, consumed 152·48 measures of oxygen, and formed 101·89 measures of carbonic acid. Hence it was composed by weight of the following constituents :—

Carbon	63·86
Oxygen	22·01
Hydrogen	14·13
	100·00

These products give us 11·21 parts of hydrogen in excess above 24·93 parts of water reduced to its elements.

Two-thirds of the gas obtained by the distillation of ether had nearly the composition just stated.

When the oxycarbureted hydrogen was lightest, the cubic deci-

metre (61·028 cubic inches) weighed 0·81196 gramme (12·54 grs.) at the same temperature and pressure, and in the same degree of dryness as the preceding : 100 measures of it consumed 143·32 measures of oxygen gas, and formed 93·15 measures of carbonic acid gas. Hence it follows that 100 parts by weight of this gas are composed of the following constituents :—

Carbon .... 61·70
Oxygen .... 23·49
Hydrogen .... 14·81
──────
100·00

In these products we find an excess of 11·7 hydrogen above the 26·6 parts of water reduced to its elements.

This gas, notwithstanding its variation in its specific gravity, has this property in common with alcohol and with the oxycarbureted hydrogen obtained by the decomposition of alcohol, that *the oxygen consumed is to the carbonic acid produced as three to two.* It follows that the oxycarbureted hydrogen gas from ether is represented, as in all the preceding analyses, by olefiant gas and water. The oxycarbureted hydrogen from alcohol is represented by 2 olefiant and nearly 1 water, or by 100 olefiant gas and 47·6 water.

The oxycarbureted hydrogen from ether is represented by 3 olefiant gas and 1 water in weight.

8. *Analysis of Sulphuric Ether by the Detonation of its elastic Vapour with Oxygen Gas.*

In my first experiments on the composition of ether I analysed it by saturating oxygen gas with the vapour of ether. The superabundant liquid was removed by filtering paper, the increase of bulk was determined, and the whole was detonated, after the addition of a farther portion of oxygen gas. The first operations were independent of each other, and they required the same temperature and pressure, which could not be ascertained in a satisfactory manner. When I absorbed the liquid ether by means of filtering paper I was not aware that any of the ether in the state of vapour was condensed; but new experiments on the absorption of gases by porous bodies have shown me that this is the case, though in other respects paper has no action on ether.

I follow at present a simpler process, and one which is much more accurate. It is sufficient, 1. To weigh in a small phial with a wide mouth, and shut with a glass stopper, a quantity of ether much less than what is sufficient to saturate the oxygen gas, which ought to stand over mercury in a tall narrow glass jar. 2. To open the phial under mercury, and introduce it into the receiver. 3. To measure the volume of the gas after the solution of the ether in the oxygen. We thus obtain an etherial gas which does not contain liquid ether, and which may be analysed with accuracy, as we know the proportion of oxygen and ether gas which it contains.

I add, for example, 0·525 gramme* (8·108 grains) of liquid ether to 526·76 cubic centimetres (3214·7 cubic inches) of dry oxygen gas, reduced to the pressure of 29·922 inches of mercury, and at the temperature of 32°. In half an hour all the liquid ether disappeared, and the gas dilated by the etherial vapour occupied the bulk of 682·87 cubic centimetres (4167·4 cubic inches).

I put this into the detonating tube over mercury, and burned it by means of the electric spark, after adding to it four times its volume of oxygen gas; for unless there be a great proportion of oxygen, the eudiometer is broken by the explosion. These 5·25 decigrammes (8·108 grains) of ether consumed in this experiment 1008·6 cubic centimetres (6155·3 cubic inches), and formed 661·4 cubic centimetres (4036·4 cubic inches) of carbonic acid gas.

In another experiment, which was more exact, because it was made with oxygen gas in a state of greater purity, 5·4 decigrammes (8·34 grains) of ether changed 525·81 cubic centimetres (3208·9 cubic inches) of oxygen gas into 687·23 cubic centimetres (4194 cubic inches). This quantity of ether consumed by its detonation 1027 cubic centimetres (6267·6 cubic inches) of oxygen gas, and formed 632·8 cubic centimetres (4167 cubic inches) of carbonic acid gas. Hence it follows that 100 parts by weight of ether are composed as follows :—

Carbon .... 67·98
Oxygen .... 17·62
Hydrogen .... 14·40
                                  ———
                                  100·00

In these products we find 12·07 parts of hydrogen above 19·95 of water reduced to its elements.

This hydrogen in excess above the elements of water is to the carbon contained in ether as 1 : 5·63; or, in other terms, *the volume of oxygen gas consumed by the ether is to the volume of carbonic acid gas produced as three to two,* as is the case in alcohol, in the oxycarbureted hydrogen gas from alcohol, in that from ether, and in olefiant gas. If we add (in the 100 parts of ether that I have analysed) to the 67·98 parts of carbon the 12·07 parts of hydrogen in excess above the elements of water we obtain 80·05 parts which are composed as olefiant gas is.† The 19·95 parts

* I might easily have made this experiment on a larger scale, but it would not have been more accurate, because the eudiometrical errors would have been infinitely greater than those of weighing, which was conducted on a balance sensible to a milligramme (0·0154 grain).

† If we substitute in these 80·05 parts the principles of olefiant gas as I obtained them in sect. 3, we find them to be—

Carbon.............................68·07
Hydrogen..........................11·98
                                  ———
                                  80·05

remaining are merely water. *Ether, then, is represented by the elements of* 100 *parts in weight of olefiant gas and* 25 *parts of water.*

By the analysis which I have made we perceive the justice of Count Rumford's observations in his Inquiry into the Heat evolved during Combustion. This philosopher, on comparing the heat produced by the combustion of ether with the heat that would have been produced by the elements of that liquid, such as I announced them in my first experiments, (Journal de Physique, 1807) found, by means of his calorimeter, that I must have assigned an excess of hydrogen. He confirmed by this method my first analysis of alcohol : but this agreement is not incompatible with an inaccurate analysis when the errors compensate each other.

9. *Principal Results of the preceding Researches.*

Ether and alcohol are two combinations of water and olefiant gas reduced to their elements.*

The same holds with the gas obtained by passing ether or alcohol through a red-hot porcelain tube.

Ether of the specific gravity 0·7155, at the temperature of 68°, is represented by the elements of 100 parts of olefiant gas in weight and 25 parts of water.

Alcohol is represented by olefiant gas united to a much greater proportion of water, and which may be stated as an approximation at half the weight of the olefiant gas.

The alcohol of Richter, or the alcoholic liquid, having the specific gravity 0·792, at the temperature of 68°, is represented by 100 parts of olefiant gas and 63·6 parts of water. As this alcohol contains an indefinite quantity of water accidentally present, that quantity is confounded (in the 63·6 parts just indicated) with the water essential to pure alcohol. †

One hundred parts in weight of alcohol of Richter contain—

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* According to the preceding analysis ether should be a compound of five integrant particles of olefiant gas and one of water ; or of six atoms hydrogen, five carbon, and one oxygen. Hence it contains more carbon and less oxygen than alcohol, and is of a much more complicated composition.—T.

† Among the conjectures which may be made respecting the quantity of water essential to pure alcohol, the supposition which appears to me the most probable is that it contains a quantity of elementary water double in weight to that which exists in ether ; that is to say, that pure alcohol is represented by 100 olefiant gas and 50 water, as is nearly the case with the gas obtained by passing aqueous alcohol through a red-hot porcelain tube. According to this notion, 100 parts of Richter's alcohol would contain 91·7 pure alcohol and 8·3 water ; and 100 of pure alcohol would be composed of

Carbon......................................	56·68
Oxygen......................................	29·44
Hydrogen....................................	13·88
	100·00

In these products there is an excess of 9·98 hydrogen above the water reduced to its elements.

Carbon .... 51·98
Oxygen .... 34·32
Hydrogen .... 13·70
                                   ———
                                   100·00

In these products there are 9·15 parts of hydrogen in excess above 38·87 parts of water.

One hundred parts in weight of sulphuric ether, of the specific gravity 0·7155, at the temperature of 68°, contain—

Carbon .... 67·98
Oxygen .... 17·62
Hydrogen .... 14·40
                                   ———
                                   100·00

In these products we find 12·07 parts of hydrogen in excess above 19·95 parts of water reduced to their elements.

The analysis of ether is more precise than that of alcohol, as far as the proportion of water is concerned, not only because the ether has been analysed by a more direct process, but because water being almost insoluble in ether, it can only contain a very small quantity of water accidentally present.

To obtain the proportion of olefiant gas in alcohol, ether, and the gases produced by passing these liquids through a red-hot tube, it is sufficient to add to the carbon indicated by analysis the excess of hydrogen above the elements of water. The remainder is merely water. Thus the weight of the olefiant gas in 100 parts of ether is equal to 67·98 + 12·07 = 80·05 ; and the 19·95 parts that remain are water.

The volume of oxygen gas, which alcohol consumes in burning, is to the volume of carbonic acid gas produced at the same time in the proportion of three to two, as in the case of olefiant gas, the small errors in such observations excepted. A gramme (15·444 grains) of alcohol of Richter consumes 1449·8 cubic centimetres (8848·37 cubic inches) of oxygen gas, forming 966·54 cubic centimetres (5898·77 cubic inches) of carbonic acid gas, supposing these gases at the temperature of 32°, under a pressure of 29·922 inches of mercury, and destitute of moisture.

The same ratio of three to two is observed between the oxygen consumed and carbonic acid formed in the combustion of ether. A gramme (15·444 grains) of ether consumes 1902 cubic centimetres (11607 cubic inches) of oxygen gas, and forms 1264 cubic centimetres (7713·9 cubic inches) of carbonic acid gas.

The same ratio of three to two exists likewise in the products of the combustion of the gases, derived from the decomposition both of alcohol and ether : 100 parts in volume of the gas from alcohol consume 121·95 measures of oxygen gas, and form 81·15 measures

of carbonic acid gas. The cubic decimetre (61·028 cubic inches) of this gas weighs 0·75804 gramme (11·707 grains).

One hundred parts by weight of this gas are composed of

Carbon .... 57·574 ⎫ 10·189 hydrogen in excess above
Oxygen ... 28·466 ⎬ 32·237 parts of water reduced to its
Hydrogen. . 13·960 ⎭ elements

100·000

One hundred parts in volume of the gas from the decomposition of ether consume 152·48 measures of oxygen, and form 101·89 measures of carbonic acid. The cubic decimetre (61·028 cubic inches) of this gas weighs 0·85808 gramme (13·252 grains).

One hundred parts in weight of this gas contain the following constituents :—

Carbon .... 63·86 ⎫ 11·21 hydrogen in excess above
Oxygen ... 22·01 ⎬ 24·93 parts of water reduced to its
Hydrogen. . 14·13 ⎭ elements

100·00

The volume of hydrogen gas in excess above the elements of water, both in alcohol, ether, their gaseous products, and in olefiant gas, is equal to the volume of the carbonic acid gas produced by each of these substances during its combustion.

We may in consequence of this property, or of the ratio of three to two mentioned above, establish the proportion of the three elements which constitute the known weight of alcohol or ether, if we have any one of the three following data :—1. The hydrogen in excess above the elements of the water. 2. The oxygen consumed during the combustion. 3. The acid produced during that process.

Since alcohol is represented by olefiant gas united to a quantity of water equal to about half the weight of that gas, while ether is composed of olefiant gas united only with water amounting to one-fourth the weight of that gas, and since olefiant gas contains no water, we see what is the action of sulphuric acid on alcohol to form either ether or olefiant gas. In both cases it merely abstracts the water essential to the alcohol. When we mix this last liquid with about its own weight of sulphuric acid, we employ only the quantity of acid necessary to abstract one-half of the water essential to ether, and we obtain ether. When we mix alcohol with four times its weight of sulphuric acid, we add a sufficient quantity to deprive it of the whole of its water, and then, instead of ether, olefiant gas is obtained.

A certain portion of the alcohol destined to these operations appears, in consequence of the too energetic action of the acid, to undergo a farther decomposition towards the end of the distillation than is necessary either for the production of ether or olefiant gas.

Hence the charry residue. This decomposition is the cause that we obtain a much smaller quantity of ether than that contained in the elements of the alcohol. Two parts of the alcohol of Richter contain the elements of about one and a half of ether; but we never obtain more than two-thirds of this quantity.*

ARTICLE VI.

Catalogue of Scientific Books published in Sicily between 1800 *and* 1812.†

I. *Works on Physics and Astronomy.*

1. Elements of Experimental Philosopy; by the Abbate Scina; Palermo, 1803; in three volumes, with plates.

2. Elements of particular Physics (Fisica particolare); by the same author; Palermo, 1804; one volume.

3. The Foundation of general Physics, established on the Evidence of Facts; by the Abbate Russo and Scire; Palermo, 1810; one volume.

4. Mean Position of the principal fixed Stars at the beginning of the Nineteenth Century; by P. Giuseppe Piazzi; Palermo, 1803; one volume folio.

5. A Dissertation on the Comet of 1811; by P. Giuseppe Piazzi; Palermo, 1811.

6. Annual Calendar; by P. Piazzi; Palermo, 1806—1814.

II. *Works in Natural History and Botany.*

7. Elements of Natural History; by Professor Camcillo; Palermo, 1801; three volumes, and a Supplement to the Mineralogy.

8. Memoirs by the Abbate Francesco Ferrara, of Catania: 1. On the Bitumen Lake in the Southern part of Sicily: 2. On Sicilian Amber: 3. On Hiblean Honey, and the city of Hibla Megara: 4. On Nasso and Gallipoli; Palermo, 1805; one volume. This book is written by the well known author of the History of Etna.

9. The Flegrean Plains of Sicily; by the Abbate Ferrara, Professor of Natural Philosophy in the University of Catania; Messina, 1810; one volume, with figures and a map.

10. Characters of some new Genera and Species of Animals and

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* This explanation of Saussure does not appear to correspond quite accurately with fact. When a mixture of four parts sulphuric acid and one part alcohol are distilled, not only olefiant gas is evolved, but likewise carbonic acid gas; and the proportion of this last gas increases as the process advances.—T.

† I have translated this article, which the English reader will probably consider as a curiosity, from an Italian journal published at Palermo monthly, and entitled, Specchio delle Scienze o Giornale Enciclopedico di Sicilia. I have received the first two numbers, published in January and February, 1814.—T.

Plants found in Sicily, with various Observations on the same; by
Sign. Constantino S. Rafinesque Schmaltz; Palermo, 1810; one
volume, with 20 plates, and 77 figures. This book contains a
description of 24 new species of birds and reptiles, 51 new genera
and 154 new species of fish, 21 new genera and 88 new species of
plants, all observed in Sicily, and hitherto undescribed.

11. Index to the Echthyology of Sicily, or a methodic catalogue
of the Latin, Italian, and Sicilian names of the fishes which
frequent the coast of Sicily, arranged according to a natural
method, and followed by an appendix which contains the descrip-
tion of some new Sicilian fishes; by Sign. C. S. Rafinesque
Schmaltz; Messina, 1810; one volume, with two plates. This
book contains a description of 390 species of Sicilian fish, 200 only
of which have been described by preceding writers. The appendix
contains a description of 28 new genera and 45 new species.

12. A hundred and twenty plates of the Panphyton Siculum of
Cupani, newly engraved; by Sign. C. S. Rafinesque Schmaltz;
Palermo, 1812; folio. The original work of Cupani contains 650
plates; 120 of them have been engraved, to give the Sicilians an
idea of the original work.

13. Prospectus of the Pamphysis Sicula, sive Historia Naturalis
Animalium, Vegetabilium, et Mineralium quæ in Sicilia vel in
circuitu ejus inveniuntur. Opus incæptum a P. Franc. Cupani in
Panphyto Siculo, continuatum ab Anton. Bonanno Gervasi, Jos.
Steph. et Franc. Chiarellius, et ab C. T. Rafinesque Schmaltz locu-
pletatum, &c.; Palermo, 1807.

14. Fifty Figures of new Genera and new Species of Plants of
the United States of America; by Sign. C. S. Rafinesque Schmaltz;
Palermo, 1808.

15. Sicularum Plantarum Centuria Prima; by Antonino Bivona
Bernardi; Panormi, 1806—Centuria Secunda, Panormi, 1807.

16. A Monograph of the Genus *Talpis*; by Ant. Bivona;
Palermo, 1810.

17. Essay on Botany; by Ferdinando Cosentino; Catania, 1805.

18. Prospectus of the Minerals of Sicily; by Dr. Giuseppe
Emmanuele Ortolani; Palermo, first edition, 1808—second edition,
1809.

19. Synoptical Tables of the Productions of Etna; by Dr. Car-
melo Moravigna; Catania, 1810.

20. Synopsis Plantarum Horti Botanici Academia Regia Panor-
mitanæ; by Dr. Giuseppe Tineo; Panormi, 1802 and 1807.

21. Account of the Eruption of Etna in 1802; by Dr. Mara-
vigna, Professor of Chemistry in the University of Catania;
Catania, 1803.

22. Account of the Eruption of Etna in 1809; by the Abate
Chiavetta; Messina, 1809.

23. Account of the same Eruptions of 1809; by Sign. Gem-
mellaro; Messina, 1810.

24. Description of three new Sicilian Plants, *Astragalus Siculus,*

Anthoxanthum Gracile, and *Barkausia Hyemalis*; by Sign. Ant. Bivona. In the Literary and Political Journal of Palermo, 1810, Nos. 4, 8, and 10.

III. *Works on Agriculture and Economy.*

25. Three Memoirs on Agriculture; by the Cav. Livy. 1. On the Use of the Alga Marina in Agriculture and in the Arts. 2. On the Treatment of Vines. 3. On the Economy of the Flour which flies away in the Mill, and during the Separation of the Bran; Palermo, 1800.

26. Economical and Agricultural Memoirs regarding the Corn of the Kingdom of Sicily; by the Ab. Paolo Balsamo; Palermo, 1803.

27. Journal of a Journey made in Sicily, and particularly in the County of Modica; by the Ab. Paolo Balsamo; Palermo, 1809.

28. Instructions respecting the Culture of Mulberries and the Breeding of Silk Worms, by means of which a greater Quantity may be obtained, with less Expense and Trouble; by Antonio Maria Curiazzo; Messina, 1810.

29. Agricultural Essays adapted to the Island of Malta and Gozzo; by P. Carlo Giacinto, Professor of Botany in Malta; Messina, 1811.

30. Economical Dissertations respecting the Kingdom of Sicily; by Sign. Salvadore Scuderi, Professor of Economics, Commerce, and Agriculture, in the University of Catania; Catania, 1811.

IV. *Works on Medicine.*

31. Program of a System of Theoretic Medicine, arranged according to the Analytical Method; by Dr. Rosario Scuderi; Palermo, 1804.

32. Dissertation on the Extirpation of Cancers; by Dr. Franc. Cannizzaro di Vizzini; Palermo, 1801.

33. Advice to Physicians respecting the System of Brown; by Dr. Trusso; Palermo, 1806.

34. On the Reforms requisite in the Medical Chairs of the University of Palermo; by Dr. Vincenzo Genuardo; Palermo, 1809.

35. A Historical Essay on the Yellow Fever of America; by Dr Schiofessie, translated by Dr. Coco; Palermo, 1805.

36. Medical Considerations and Meditations on Living Man; by Dr. ——; translated by Dr. Carmelo Maravigna, with notes; Catania, 1810.

37. Essay on Misfortune and its Medicinal Virtues; by Sign. Comandante Poli; Palermo, 1811.

38. Essay on the Cow-Pox; by Dr. Francesco Calcagni, Phy-

sician of the Spedale Grande; to serve for advice to the people and instructions to young physicians and surgeons to practise or direct inoculation, and preserve the matter; Palermo, 1809.

39. Essay on the Cow-Pox found in the Bocca di Falco, a village near Palermo, in the month of April, 1811; by Dr. Giov. Battista Amati; Palermo, 1811.

40. Medical Education; by the Abate D. Ignazio Salemi. An association was proposed in Palermo in 1811 for printing this work; but the undertaking has not yet been accomplished. The same year Dr. Calcagni proposed to print a translation of Bichat's General Anatomy applied to Physiology and Medicine; but the expense could not be defrayed, and the manuscript still remains in the hands of the translator.

V. *Works on Geography and Statistics.*

41. Geographical Dictionary of the Kingdom of Sicily; by the Abate Francesco Sacco; Palermo, 1800.

42. Geographical Description of the Island of Sicily and the neighbouring Islands; by Sign. Mariano Scasso Borello; Palermo, 1810.

43. General Statistics of Sicily; by Messrs. Ortolani and Rafanesque : Part First, Physical Description of Sicily ; Palermo, 1810.

The remainder of this work was suppressed by the Sicilian Government, because it laid open the abuses under which Sicily groans.

44. An Historical and Statistical Essay on the City and Island of Zante; by Sign. Paolo Mercati; Messina, 1811.

45. Elucidations of the Map of the Old City of Jerusalem; by P. Tommaso Rodriguez, Commissioner to the Holy Land; Messina, 1812.

Article VII.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North.   Longitude West in Time 6″$\frac{8.9}{100}$.

May 18, Emersion of Jupiter's	⎧10ʰ	35′	04″	Mean Time at H.W.
3d Satellite	⎩10	35	13	Ditto at Greenwich.
May 25, Immersion of Jupiter's	⎧11	18	47	Mean Time at H.W.
3d Satellite	⎩11	18	44	Ditto at Greenwich.

Magnetical Observations.

1814.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
May 18	8ʰ 45′	24° 13′ 17″	1ʰ 55′	24° 23′ 43″	6ʰ 30′	24° 15′ 50″
Ditto 19	8 45	24 12 34	1 45	24 22 04	6 35	24 15 12
Ditto 20	8 50	24 16 25	1 35	24 22 41	6 30	24 16 00
Ditto 21	8 40	24 12 11	1 40	24 24 34	6 50	24 18 49
Ditto 22	8 45	24 9 22	1 35	24 25 10	6 35	24 16 38
Ditto 23	8 35	24 9 30	— — —		6 40	24 11 16
Ditto 25	8 35	24 10 02	1 35	24 20 18	8 05	24 15 58
Ditto 26	8 45	24 15 20	1 50	24 22 25	6 35	24 16 11
Ditto 27	8 45	24 13 56	1 35	24 20 55	6 35	24 17 08
Ditto 28	8 45	24 14 20	1 35	24 20 30	6 35	24 16 58
Ditto 29	8 45	24 13 41	1 45	24 23 10	6 30	24 17 03
Ditto 30	8 45	24 11 56	1 45	24 23 01	6 30	24 15 35
Ditto 31	8 45	24 14 55	1 40	24 23 47	6 40	24 16 38

1814.

Mean of Observations in May.	{	Morning	at	8ʰ 45′ ..... Variation	24°	12′	49″	} West.
		Noon	at	1 44 ..... Ditto	24	22	13	
		Evening	at	6 38 ..... Ditto	24	16	14	
Ditto in April.	{	Morning	at	8 45 ..... Ditto	24	12	53	} West.
		Noon	at	1 48 ..... Ditto	24	23	53	
		Evening	at	6 29 ..... Ditto	24	15	30	
Ditto in March	{	Morning	at	8 52 ..... Ditto	24	14	29	} West.
		Noon	at	1 52 ..... Ditto	24	23	08	
		Evening	at	6 11 ..... Ditto	24	15	33	
Ditto in Feb.	{	Morning	at	8 47 ..... Ditto	24	14	50	} West.
		Noon	at	1 52 ..... Ditto	24	20	58	
		Evening	at —	— ..... Ditto	—	—	—	Not obs.
Ditto in Jan.	{	Morning	at	8 52 ..... Ditto	24	15	05	} West.
		Noon	at	1 53 ..... Ditto	24	19	03	
		Evening	at —	— ..... Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	{	Morning	at	8 53 ..... Ditto	24	17	21	} West.
		Noon	at	1 53 ..... Ditto	24	19	49	
		Evening	at —	— ..... Ditto	—	—	—	Not obs.
Ditto in Nov.	{	Morning	at	8 42 ..... Ditto	24	17	42	} West.
		Noon	at	1 54 ..... Ditto	24	20	24	
		Evening	at —	— ..... Ditto	—	—	—	Not obs.
Ditto in Oct.	{	Morning	at	8 45 ..... Ditto	24	15	41	} West.
		Noon	at	1 59 ..... Ditto	24	22	53	
		Evening	at —	— ..... Ditto	—	—	—	Not obs.
Ditto in Sept.	{	Morning	at	8 53 ..... Ditto	24	15	46	} West.
		Noon	at	2 02 ..... Ditto	24	22	32	
		Evening	at	6 03 ..... Ditto	24	16	04	
Ditto in Aug.	{	Morning	at	8 44 ..... Ditto	24	15	58	} West.
		Noon	at	2 02 ..... Ditto	24	23	32	
		Evening	at	7 05 ..... Ditto	24	16	08	
Ditto in July.	{	Morning	at	8 37 ..... Ditto	24	14	32	} West.
		Noon	at	1 50 ..... Ditto	24	23	04	
		Evening	at	7 08 ..... Ditto	24	13	56	
Ditto in June.	{	Morning	at	8 30 ..... Ditto	24	12	35	} West.
		Noon	at	1 33 ..... Ditto	24	22	17	
		Evening	at	7 04 ..... Ditto	24	16	04	
Ditto in May.	{	Morning	at	8 22 ..... Ditto	24	12	02	} West.
		Noon	at	1 37 ..... Ditto	24	20	54	
		Evening	at	6 14 ..... Ditto	24	13	47	

Ditto in April. { Morning   at 8   31 ..... Ditto   24   09   18 } West.
               { Noon   at 0   59 ..... Ditto   24   21   12 }
               { Evening   at 5   46 ..... Ditto   24   15   25 }

Magnetical Observations continued.

Month.	Morning Observ.				Noon Observ.				Evening Observ.			
	Hour.	Variation.			Hour.	Variation.			Hour.	Variation.		
June 1	8^h 40$'$	24$°$	14$'$	36$''$	1^h 50$'$	24$°$	23$'$	25$''$	—^{h} —$'$	—$°$	—$'$	—$''$
Ditto 2	— —	—	—	—	—	—	—	—	6 35	24	17	50
Ditto 3	8 40	24	15	32	— —	—	—	—	— —	—	—	—
Ditto 4	— —	—	—	—	1 35	24	23	38	— —	—	—	—
Ditto 5	8 50	24	14	00	2 25	24	24	56	6 45	24	15	49
Ditto 6	8 40	24	14	05	1 45	24	21	57	6 40	24	18	10
Ditto 7	8 50	24	10	58	2 16	24	22	59	6 40	24	17	50
Ditto 8	8 40	24	13	56	1 35	24	21	58	6 40	24	18	41
Ditto 9	8 95	24	13	02	1 30	24	23	47	6 45	24	17	13
Ditto 10	8 45	24	13	58	1 35	24	22	39	6 50	24	15	50
Ditto 11	8 40	24	14	23	1 40	24	23	26	6 45	24	16	16
Ditto 12	8 30	24	13	07	1 55	24	22	28	8 10	24	15	02
Ditto 13	8 45	24	9	15	1 45	24	23	28	6 55	24	14	15
Ditto 14	8 35	24	10	44	1 35	24	23	20	7 05	24	16	41
Ditto 15	— —	—	—	—	1 35	24	24	18	6 50	24	17	26
Ditto 16	8 30	24	11	33	12 50	24	23	26	6 45	24	15	03
Ditto 17	8 40	24	12	33	1 50	24	21	50	6 45	24	15	38

Rain fallen { Between noon of the 1st May } 2·294 inches.
             { Between noon of the 1st June }

Errata in the Noon Observation of May 17, 1814.
For 24$°$ 11$'$ 52$''$ read 34$°$ 21$'$ 52$''$

ARTICLE VIII.

ANALYSES OF BOOKS.

Experiments and Observations on the Atomic Theory and Electrical Phenomena. By William Higgins, Esq. F.R.S. and M.R.I.A. Professor of Chemistry to the Dublin Society.

WHEN I published my answer to Mr. Nash's claim of the discovery of the atomic theory for Mr. Higgins, in the *Annals of Philosophy* for May, I was not aware of the publication of the book the title of which stands at the head of this article, and still less of the great heat with which Mr. Higgins himself had set forward his own claim. The work was printed in Dublin, for Longman and Co. London. It was put into my hands by a friend some weeks ago. I have never seen it advertised in any newspaper; yet there can be no doubt that the book is published and on sale in London. I

conceive, therefore, that I shall contribute in no inconsiderable degree to promote the object of the author by giving an account of it here; which will make its existence known to the chemical world in general very speedily.

The book itself consists of 172 octavo pages; there is, besides, an Appendix of eight pages, very strongly resembling Mr. Nash's letter in the Philosophical Magazine, and confirming some suspicions which had struck me respecting the source from which the statements in that letter had originated. I shall in the first place endeavour to give my readers a notion of the work itself, and then consider the Appendix.

The book itself consists of three parts, not very intimately connected with each other. 1. Copious quotations from his former work, published in 1789. These quotations are modernized in the language, and adapted to the present chemical nomenclature. I have not taken the trouble to compare them with the original; but as far as I can judge from recollection, they seem to be pretty fair translations of the passages in the Comparative View. The object of these quotations is to show that Mr. Higgins, in the Comparative View, established three points: 1. That chemical substances unite atom to atom. 2. That when an atom of any body, *a*, combines with two atoms of another body, *b*, it retains one atom of *b* with twice the force that it does two atoms. In like manner it retains one atom of *b* with thrice the force that it does three atoms, and with four times the force that it does four atoms. 3. That in his work, entitled, Comparative View, he pointed out the method of determining the weight of atoms, and did every thing that Mr. Dalton did, and a great deal more.

" I cannot," he observes, " with propriety or delicacy, directly say that Mr. Dalton is a plagiarist, although appearances are against him. Probably he never read my book; yet it appears extraordinary that a person of Mr. Dalton's industry and learning should neglect one of the few works that was expressly written on the subject of theory. At the time it was published, there were one thousand copies of it sold, and it was the principal means of putting an end to the controversy already alluded to (*the phlogistic controversy*). which otherwise might not be determined for many years." P. 10.

In another place he says—" Mr. Dalton's work is read, mine had been laid aside as soon as the controversy which gave rise to it ceased; and at that time the theory in question was not understood, nor did I expect it would for a considerable time. I calculated upon the middle of the present century. Probably it would have lain by since (*till then*), if it had not been for the genius and industry of Mr. Dalton." P. 17.

Now I do not know what opinion Mr. Higgins may form of these two passages, but to me they appear rather inconsistent with each other. If the doctrine in question was clearly taught in Mr. Higgins' Comparative View, how came it not to be understood?

And if it was not understood, how should Mr. Dalton be accused of plagiarism for teaching a doctrine which Mr. Higgins, and Mr. Higgins only, was capable of knowing to be the same which he had taught? If Mr. Dalton was able to solve the enigma, his merit was at least equal to that of Mr. Higgins; and as he thought proper to communicate it to the world in an intelligible manner, chemists in general are certainly indebted to him for the whole of the doctrine, even if it should prove true that this doctrine was previously known to Mr. Higgins.

As far as I have had an opportunity of judging, Mr. Higgins' Comparative View was very little known to chemists in general till Sir Humphry Davy published his note, claiming for it the discovery of the *atomic theory.* I myself met with a copy of it by accident in 1798, I never met with a single person in Edinburgh who had read it; nor were any of the London chemists, as far as my knowledge goes, acquainted with it before Mr. Higgins himself pointed it out to Sir H. Davy, as containing the outline of the atomic theory. I have never seen any reference to it during the phlogistic controversy; and I always understood that the prevalence of the Lavoiserian theory in Britain was owing entirely to the refutation of Kirwan's Essay on Phlogiston by the French chemists. I would not be understood by these observations to insinuate any thing against the merit of Mr. Higgins' work, which I consider as a highly ingenious and valuable production. It was partly owing to the subject on which it was written, and partly to the time in which it appeared, that so little notice was taken of this work. After a controversial subject is finally decided, all books upon it of necessity lose their interest, and are seldom looked at. The victory of the antiphlogistians occasioned a kind of chemical interregnum in Britain from 1790 to 1798 or 1800. The new race of chemists which then appeared had been educated in the antiphlogistic doctrine, and never of course would think of perusing a work expressly written on that exploded controversy. For my own part, as I have already said, I met with a copy of the book by accident in 1798, when I was a student at the University of Edinburgh. I read it at that time in a cursory manner, and never looked at it again till Davy's note appeared. I then read it again, and told Davy at the time that I could not find the atomic doctrine in it. I put the question soon afterwards both to Dr. Henry and Mr. Dalton, and both of them assured me that they had never seen the book. I dare say both of them have now read it.

After this discussion, which perhaps was unnecessary, but into which I entered to show Mr. Higgins that he has perhaps overcalculated the effect produced by his book, and that his insinuations of plagiarism are a little too strong, and a little too inconsistent, I shall proceed in the next place to consider the three points which he says he has established in his work. If I understand him rightly, he considers himself as the first person who taught, 1. That bodies unite atom to atom. 2. That the strength of affinity is inversely

as the number of atoms held in combination by any atom, *a.*
3. That the weight of atoms may be determined from chemical
analysis.

Now as to the first point, namely, *that bodies consist of atoms,
and that chemical combination consists in the union of one atom of
one body with one atom of another body, or of a certain determi-
nate number of the atoms of one body with a determinate number of
the atoms of another*; I am surprised that such a proposition should
be claimed by any living chemist as a discovery. It was the doctrine
of Sir Isaac Newton, as is obvious from the thirty-first query at the
end of his Optics; and I conceive it has been the uniform doctrine
of all British chemists since his time. I do not know what was the
case at Oxford, where Mr. Higgins appears to have studied; but at
Edinburgh it was universally considered as a first principle. I was
informed by Professor Robinson that it had been taught by Dr.
Cullen; and Dr. Pearson has assured me, from his own personal
knowledge, that the atomic doctrine constituted the basis of Dr.
Cullen's chemistry. Dr. Fordyce must have been a lecturer on
chemistry in London while Mr. Higgins was actively engaged in
the study of chemistry. Now Dr. Pearson informs me that he not
only taught the atomic doctrine, but was of opinion that bodies are
only capable of uniting *particle to particle,* or one particle of one
body with one particle of another. This was one of my own early
opinions, which I adopted when a student, and gave up, after the
Daltonian theory induced me to examine the subject with more
precision than I had previously done. This being the matter of
fact, and these the views which I entertained when I read Mr.
Higgins' book, he will not be surprised when I inform him that I
found nothing new in it as far as this point, which he seems to
claim as a discovery exclusively his own, is concerned. If after
reading this statement he is not satisfied that the atomic doctrine
was known before the publication of his book, I would advise him
to peruse the article *Affinity* in the Encyclopedie Methodique,
which was written several years before his Comparative View. Let
him read likewise the first section of Bergman's treatise on Elective
Attraction, which was translated into English by his friend Dr.
Beddoes.

As to the second point which Mr. Higgins claims as a discovery
of his own, that the strength of affinity is inversely as the number
of atoms united, I have it not at present in my power to consult
those works, in which I think I recollect that the doctrine is taught,
and which were published before his Comparative View. I believe
it is explicitly taught somewhere in the first volume of the chemical
part of the Encyclopedie Methodique, which is of prior date to
that publication; but whether under the head of *Affinité*, or not,
I am not sure, as I have not had the work in my possession for
many years. It was a doctrine which was familiar to me before I
saw Mr. Higgins' work, a doctrine which I had imbibed with the
first rudiments of my chemical knowledge. He will not be sur-

prised therefore, when I assure him that as far as that doctrine is concerned, I found nothing which to me appeared new in his book.

As to the third point, I consider it as the only material one; for in my opinion the art of weighing the atoms of bodies is the only addition which Mr. Dalton has made to the atomic theory; but it is an addition of the utmost importance, as it has set that theory in a new point of view, and rendered it, from being a naked and insulated and useless fact, the very basis and first principle of the whole science. Accordingly it is to the few years that have elapsed since that method was pointed out by Mr. Dalton that we are indebted for almost all the accurate knowledge that we possess of the composition of bodies. If Mr. Higgins, then, can make good his claim to the discovery of determining the weight of the atoms of bodies, the whole atomic theory will be his exclusively. Nay, if he can show in his book the least hint at the weight of any one atom, or that the weight of an atom can be ascertained, I shall very readily admit the whole of his claim.

In page 11 of the work, which I am at present examining, Mr. Higgins says, "The relative weights of the different particles of elementary matter, that of hydrogen being a standard, have also been given by Mr. Dalton. I have done the same in many instances. After this, the relative weight of compound atoms could readily be conceived, and Mr. Dalton has extended this to saline substances." Such is the assertion of Mr. Higgins, which he repeats in various other passages of his book. Now after reading over the whole of his quotations with attention, I find nothing but the following passages on which he seems to found the whole of these assertions :—

Sulphuric " acid, exclusive of water, consists of two parts of oxygen and one of sulphur by weight.

" One hundred and forty-three grains of oxygen gas contain 41 of water; quicklime will abstract 26 grains from it, and the remainder of its water cannot be separated by similar means; therefore 100 grains of sulphur require only 100 or 102 of the dry gravitating matter of oxygen gas to form sulphurous acid. As sulphurous acid gas is very little more than double the specific gravity of oxygen gas, we may conclude that the ultimate particles of sulphur and oxygen contain the same quantity of matter; for oxygen gas suffers no considerable diminution of its bulk by uniting to the quantity of sulphur necessary to form sulphurous acid. *It contracts* $\frac{1}{11}$, *as shall be shown hereafter.*

" Hence we may conclude that an atom of sulphurous acid consists of a single particle of oxygen and a single particle of sulphur, chemically united; and that every molecule of sulphuric acid contains one particle of sulphur and two of oxygen, the proportion necessary to saturation.

" As two cubic inches of hydrogen gas require but one cubic inch of oxygen gas to condense them to water, we may presume that they contain an equal number of divisions, and that the differ-

ence of the specific gravity of those gases depends on the size of their respective particles; or we must suppose that an ultimate particle of hydrogen requires two, or three, or more particles of oxygen to saturate it. Were this the case, water or its constituents might be obtained in an intermediate state of combination, like those of sulphur and oxygen, azote and oxygen, &c. This appears to be impossible; for in whatever proportion we mix hydrogen and oxygen gases, or under whatever circumstances we unite them, the result is invariably the same.

" When water is decomposed, or resolved into its constituent gases by the *voltaic battery*, or by an electric machine, the above proportions are constantly obtained.

" From those circumstances we have sufficient reason to conclude that water is composed of a single ultimate particle of oxygen and an ultimate particle of hydrogen, and that its atoms are incapable of uniting to a third particle of either of their constituents."

This passage is taken, with a slight alteration, from the Comparative View, page 36, and is given in page 57, &c. of the work at present under our consideration. Mr. Higgins terminates it with the following observations, to which the attention of the reader is requested :—

" It will, I should suppose, be needless to tell my reader that the foregoing facts relative to sulphurous acid, sulphuric acid, and water, suggested the first effort of ascertaining the comparative weights of the particles of different elementary matter, and that the weight of the atoms and molecules which they produced might readily be ascertained, those facts being once established. This part of my theory Mr. Dalton strictly attends to." P. 60.

There is one other quotation which I must still make, because it is a passage on which Mr. Higgins lays the greatest stress. It is as follows :—

" It has been shown by Dr. Austin that hydrogen gas suffers no diminution or increase of its volume by the union of sulphur; therefore one-half of the mixed airs was hydrogen independent of its sulphur; that is, $4\frac{1}{2}$ of hydrogen gas. These $4\frac{1}{2}$ measures require $2\frac{1}{4}$ measures of oxygen gas to condense them into water: $2\frac{1}{4}$ more of oxygen were expended in the experiment,* two of which must exist in the sulphurous acid; and as much of the azote† disappeared, we may conclude that the remainder of the oxygen (one-fourth of a measure) was expended in the formation of nitrous acid.

" From the foregoing facts it is evident that the two measures of sulphurous gas contain two measures of oxygen; and as the specific gravity of sulphurous gas is double that of its constituents, excluding

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* The experiment alluded to was the firing a mixture of equal bulks of sulphureted hydrogen gas and oxygen gas. A quantity of sulphurous acid gas was formed by the explosion.

† This azote was owing to the impurity of the oxygen gas employed. It had been obtained from nitre.

the hydrogen, we may infer that only the sulphur of two measures united to the oxygen gas, and that the sulphur of $2\frac{1}{2}$ measures was precipitated.

" We may also infer from these data that the atoms of sulphurous gas are surrounded with as large atmospheres of caloric as the particles of oxygen gas, or that they are as far asunder; and that the number of the ultimate particles of sulphur in sulphureted hydrogen are to those of the hydrogen as nine to five."

This quotation is from p. 81 of the Comparative View, and from p. 82 of the work which we are at present considering. In this last work Mr. Higgins subjoins to it the following paragraph:—

" This was the first experiment of the kind made; and, had there been no other evidence brought forward, ought to be sufficient for the purpose, as it establishes three important facts which comprehend the whole of what has been unjustly called Dalton's theory. 1. The proportion of the constituents of an atom of sulphurous gas, and the relative weight of those elementary principles. 2. The weight of the atom, and comparative diameter of its calorific atmosphere. And, lastly, The proportion of the ultimate particles of hydrogen and of sulphur in sulphureted hydrogen gas." P. 84.

Such are the claims and assumptions of Mr. Higgins, and such the documents upon which these claims are founded. Now I deny that there is the slightest allusion to the weight of a single elementary atom, either in these passages quoted, or in any part of the Comparative View. Nor is there the slightest reason to induce us to believe that the idea of ascertaining the weight of these atoms has so much as entered into the conception of the writer. I admit, without hesitation, that supposing Mr. Higgins to have conceived the idea of determining the weight of the atoms of bodies, and to have been aware of its importance in promoting the accuracy of chemical analysis, the two preceding quotations would have enabled him with facility to have calculated the weight of an atom of hydrogen, oxygen, and sulphur. So would the analysis of carbonic acid by Lavoisier, which existed when Mr. Higgins wrote, have enabled that philosopher, had he been aware of the importance of the subject, to determine the weight of an atom of carbon; and the analysis of ammonia by Berthollet and Austin have enabled them to determine the weight of an atom of azote.

The process, supposing the idea once suggested, is one of the simplest possible: but the great merit, I conceive, lay in suggesting the idea. This was not done by Mr. Higgins, nor was any thing even stated by him which could lead to the idea even in the loosest manner. Now the opinion which I entertain is, that the only thing new or important respecting the atomic theory, either in the writings of Mr. Higgins or Mr. Dalton, is the method of determining the weights of atoms. As this method does not exist in the work of Mr. Higgins, but was fully explained by Mr. Dalton, the merit belongs, I conceive, exclusively to him.

It is not by the facility of a process, after it has once been sug-

gested, that we are to estimate its value; but by the new light which it throws upon the subject of our investigations. All the mathematical discoveries of Sir Isaac Newton were the direct consequence of his extracting the square root of $\frac{1}{2}$. Nothing at present appears easier or simpler; yet the fluctionary calculus, and the immense progress which the science has since made, were the immediate result of that operation. It had baffled the sagacity of Dr. Wallis, who was no ordinary man; but, on the contrary, next to Newton, the most original mathematical genius of his age. In like manner the method of determining the weight of atoms, a process in itself extremely simple, baffled the sagacity of Mr. Higgins, but was accomplished by Mr. Dalton.

I must be permitted to say that, as far as my scientific knowledge goes, there never was a more unwarrantable claim made since the days of Aristotle than this claim of Mr. Higgins; and I cannot see without regret a man of his respectability and talents engaged in a dispute, from which he has put it out of his own power ever to retract. He must persevere in his allegations to the end. But the Comparative View stands in his way as an evidence against him, which no assertions on his part, or those of his friends, no display of eloquence, however pathetic, can ever overcome.

There is one other point on which Mr. Higgins lays considerable stress that I had almost forgotten to notice. He employs, in his diagrams and his reasoning, figures to denote the strength of affinity of different bodies for each other. This he calls mathematical demonstration, considers it as his own invention, and affirms that chemistry will never make effectual progress till it becomes general. I beg leave to observe, that the same thing was done long before him by Dr. Black and by Bergman, as may be seen in his diagrams of double decompositions in his well known essay on elective attractions. Morveau published a table expressing by numbers the affinities of different bodies, in the first volume of the chemical part of the Encyclopedie Methodique, several years before the Comparative View appeared. As to the originality of the idea, therefore, there can be no doubt that it does not belong to Mr. Higgins. As to the value of the practice, I own myself of a different opinion from our author. The numbers are merely arbitrary; hence they give a greater appearance of precision to our chemical reasonings than they are warranted to assume; and as it is next to impossible to pitch upon numbers that represent the real strength of affinity exerted by the bodies in question, these numbers must frequently mislead us, and induce us to draw results contrary to truth. I soon found this to be the case with Morveau's table. I was at the trouble to construct another, which I fancied was more accurate, but soon met with cases in which it likewise was erroneous. Hence I discarded numbers altogether. It is needless to observe that empirical numbers, taken at random, never can lead to any thing entitled to the name of a legitimate mathematical demonstration.

I will just hint at another thing upon which Mr. Higgins has

entered, I think, without sufficient consideration. He has given the number of atoms which combine in several instances in his Comparative View. Thus water is composed of one atom of hydrogen and one of oxygen, and sulphurous acid of one atom sulphur and one atom oxygen. Now he affirms that his numbers, where he has given them ; namely, the constituents of water, sulphurous and sulphuric acids, sulphureted hydrogen, and the compounds of azote and oxygen, are more accurate than those given by modern chemists ; for example, he still insists upon it that sulphurous acid is a compound of 1 atom sulphur + 1 atom oxygen, and sulphuric acid of 1 atom sulphur + 2 atoms oxygen. Now if this be true, an atom of sulphur and an atom of oxygen have the same weight, and these two acids are composed by weight of the following proportions :—

	Sulphur.	Oxygen.
Sulphurous acid	100 +	100
Sulphuric acid	100 +	200

Now there cannot be the least doubt that sulphuric acid is composed of 100 sulphur + 150 oxygen, proportions totally irreconcileable with his determination.

I have dwelt so long upon this controversial subject, that I have only room slightly to notice those parts of the work which are original. The electrical hypothesis consists in supposing that, as electricity passes through bodies, it displaces their specific heat. Hence the electric light. Those bodies which have but little heat are non-conductors, as is the case with glass; but when glass is heated, it becomes a conductor, in consequence of the heat thrown into it. If this hypothesis were correct, the metals ought to be non-conductors, for they have very little specific heat ; and water and hydrogen ought to be the best conductors in nature, as they have the highest specific heats. This hypothesis, I fear, would not stand the test of the slighest examination.

The original experiments relate chiefly to the proportion of oxygen necessary to consume completely a given bulk of sulphureted hydrogen ; but the sulphureted hydrogen used by Mr. Higgins was of necessity impure, as he procured it from sulphuret of iron ; at least I have made numerous experiments on the subject, and never yet extracted sulphureted hydrogen from artificial sulphuret of iron, without finding it mixed with hydrogen gas. I have in some cases found the proportion of this last gas 60 per cent., and seldom less than 10 per cent. Hence experiments with gas prepared in this manner are not entitled to confidence. Crude antimony and muriatic acid, when heated together, yield sulphureted hydrogen perfectly pure. It ought, therefore, to be always prepared in that manner for experiment.

It is easy to determine from theory how much oxygen gas is required to consume a given bulk of sulphureted hydrogen gas, supposing the sulphur to be totally converted into sulphurous acid,

which in my experiments I found to be the case : 100 measures of sulphureted hydrogen gas require for complete combustion 150 measures of oxygen : 50 of these will combine with hydrogen and form water, and 100 will combine with sulphur and form sulphurous acid. The sulphurous acid formed will, I believe, occupy exactly 100 measures.

Mr. Higgins has made us acquainted with some new facts respecting sulphureted hydrogen gas, the most important of which is its solubility in ether.

I come now to Mr. Higgins' Appendix, which is so curious a production that I cannot avoid laying it entire before my readers. It is as follows :—

" Since writing the foregoing pages, an extract, taken from a very ingenious essay on the cause of chemical proportions, written by Berzelius, Professor of Chemistry at Stockholm, appeared in a periodical work, called *Annals of Philosophy*, &c. (for December, 1813,) of which Dr. Thomson, it seems, is the editor. The celebrated author of this essay makes the following remarks on the atomic system :—

" ' When we reflect on this cause, it is first evident that it must be of a mechanical nature; and what presents itself as the most probable idea, most conformable to our experience, is that bodies are composed of atoms or of molecules, which combine one with one, one with two, or three, four, &c.: and the laws of chemical proportions seem to result from this with such clearness and evidence, that it seems very singular that an idea so simple and probable has not only not been adopted, but not even proposed before our own days.

" ' As far as I know, the English philosopher, Mr. John Dalton, guided by the experiments of Bergman, Richter, Wenzel, Berthollet, Proust, and others, was the first person who endeavoured to establish that hypothesis.' "

Sir Humphry Davy has lately assured us that Mr. Higgins, in a book, published in 1789, established the same hypothesis. I have not seen the work of Mr. Higgins, and can only notice the circumstance on the authority of Davy.

" Here follows Dr. Thomson's remarks on this passage, in a note at the foot of the page."

' The work of Higgins on *phlogiston* is certainly possessed of much merit; and anticipated some of the most striking subsequent discoveries ; but when he wrote metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact that oxygen, &c. always unite in determinate proportions, which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, &c. just as far as it was by Higgins. The latter, indeed, states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalized the doctrine, and thought of determining the weight of the atoms of

bodies. He showed me his table of symbols, and the weight of the atoms of six or eight bodies, in 1804; and I believe the same year explained the subject in London, in a course of lectures delivered in the Royal Institution. The subject could scarcely be broached sooner. But about the same time several other persons had been struck with the numbers in my table of metallic oxides, published in my Chemistry; and the doctrine would have certainly been started by others, if Dalton had missed it.—T.'

" If Dr. Thomson thought so much of my work on phlogiston, as he, erroneously, is pleased to call it, why not take notice of it in his Chemistry? As a compiler, he should not have passed it over.

" This curious note of his accounts for the omission. He wished to leave the work itself in undisturbed oblivion; but whatever was valuable in it he generously chose to bestow on Mr. Dalton.

' *When he wrote, metallic oxides,* &c.'

" When I wrote I was as well acquainted with metallic oxides as I am at this moment;* and I was the first that established ' *the grand fact that oxygen, &c. always unite in determinate propor- tions, which are multiples of the minimum proportion,*' as almost every page of this essay, which relates to the subject, will prove.

' *The atomic theory was taught by Bergman, Cullen, Black,*' &c.

" I have read the works of those chemists repeatedly, and I have not met with a single page that relates to the atomic theory. Were these philosophers now in existence, they would shrink from the compliment with honest indignation.

' *The latter, indeed, states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes.*'

" I have also attended to their particles, and to the relative weight of the particles and atoms of the different gases, as may be seen in many pages of this essay.

' *But Mr. Dalton first generalized the doctrine,*' &c.

" The doctrine was as extensively applied by me; and what is still more important, it was founded on well chosen facts and ma- thematical demonstrations, which Dalton omitted for reasons best known to himself. In a word, it will be found that Dalton has not done justice to my doctrine, with all his ingenuity; and his attempt to weigh a few atoms, no matter how, or whether he is correct or not, gives him no claim whatever to the system, which I established several years before he or Dr. T. were known as chemical writers.

' *The subject could scarcely be broached sooner.*'

" This and the remaining part of the Doctor's note exhibit such self-evident misrepresentations, that I need not say a single word on

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* " See the following pages in this essay, viz. 70, 116, 117, and 163: and in Comp. View, 229, 230.

" It is true, at the time I wrote, I thought the ultimate particles of most metals were capable of uniting to three particles of oxygen. I am now of opinion that there are but two distinct oxides of any one metal, and that the mistake of modern writers arises from a mixture of those oxides in different proportions."

the subject, but refer the reader to the pages of this work. Indeed, I did not expect that such prejudice on the one side, and partiality on the other, should flow from the pen of so respectable a writer as Dr. Thomson.

" The generous age of chemical science is no more. In my early days it was my fortune to live at the same time, and to associate with many of the venerable fathers of our present system. In that auspicious period the ultimate and ardently expected object of research was truth, not the advancement of an individual's reputation. Philosophers were then eager to attribute the merit of discovery to its rightful owner, not to appropriate it to themselves or others. But now, in the vale of life, I am myself obliged to rescue the labours of my youth from the claims of those who have adopted them without ceremony, and who have even attempted to force them from me by means of their combined exertions. However, justice will force its way, sooner or later, against all obstacles and prejudices. The subject is not now confined to the decisions of a few individuals, but is laid before a grand tribunal, and it rests with them to give a verdict."

Such is the whole of the Appendix, which, in justice to Mr. Higgins, I thought should by no means be withheld from my readers. If they will now take the trouble to turn to the *Annals of Philosophy* for May, they will find an answer to almost all the observations which this Appendix contains. Indeed, when I was writing an answer to Mr. Nash, I suspected that I was in fact refuting the objections of Mr. Higgins himself; and this suspicion has turned out to be well founded. But I must not leave unnoticed one or two things stated or hinted at in this Appendix, which in my opinion claim and require an explicit answer.

I may just notice a mistake into which Mr. Higgins has fallen in the outset, when he says that an *extract* from Professor Berzelius' paper on the cause of chemical proportions appeared in the *Annals of Philosophy*. If he had been at the trouble to read the paper in question, and I can assure him that it is worth his perusal, or if he had perused the title of it, he would have seen that it was not an *extract of a paper*, but an original paper sent by Professor Berzelius for insertion in the *Annals of Philosophy*. I would not have noticed this mistake, which I admit to be of no consequence whatever, had it not been that Mr. Higgins expresses himself as if hurt at the wrong title which I inadvertently gave to his book. He may judge from his own error that a man may fall into such a mistake without being actuated by *disingenuous* motives, or having formed a predetermined plan to injure his reputation.

Mr. Higgins asks, if I thought so highly of his book as I say I do, why I never noticed it in my Chemistry; and adds, that as a compiler I should not have passed it over. I have no objection, since he puts the question, to give him an explicit answer.

My object, when I wrote my System of Chemistry, was to make as complete a collection as possible of all the chemical facts that

appeared well ascertained, and, as far as I could, to assign the original discoverer of each of them, and the exact source from which I drew my information. It was not possible for me to accomplish completely either of these objects. Many facts escaped my attention, and I fell into occasional mistakes in assigning the original discoverers. These omissions and mistakes, when discovered, were inserted or corrected in the subsequent editions; and indeed the alterations were so numerous as at last to injure in a very material degree the arrangement and unity of the work. I have been more profuse in my quotations of authorities, and have made a more complete collection of facts than any other chemical writer that I know; and every subsequent systematic publication that I have seen, whether it made its appearance on the Continent or in Great Britain, I do not even except the present essay of Mr. Higgins, drew very copious supplies from my volumes. Some even copied verbatim the errors of the press. My rule was only to quote authorities for facts, and only for facts well established; and I never considered a fact well established when it was coupled with a circumstance which I knew to be erroneous. Mr. Higgins' book is rather theoretical than experimental, and therefore was not quite in my way. It contains indeed experiments, and establishes the composition of several bodies with accuracy; but I could not put any confidence in these determinations, because I perceived in them obvious inaccuracies. Thus he establishes that sulphurous acid is composed of equal weights of sulphur and oxygen; but his proof depends upon the supposition that 143 grains of oxygen gas contain 41 grains of water, or more than one fourth of its weight; an opinion evidently erroneous, and of course the whole of Mr. Higgins' reasoning appeared to me inaccurate. He was accidentally right from a complication of errors; but I was not to blame in considering him as wrong.

In determining the composition of the compounds of azote and oxygen, he was also right; but this determination was founded on no experiment that I could lay hold of; nor did I admit several of the substances which he introduced as distinct compounds of azote and oxygen. Hence there was nothing tangible which I could introduce into my System.

The only experiment that I would have quoted, if I had recollected it, was the solution of iron in sulphurous acid, without the extrication of any gas. This discovery belongs to Mr. Higgins, and has been unjustly claimed by Vauquelin; though I dare say this distinguished chemist, of whose honesty I have a high opinion, was not aware of what Mr. Higgins had done when he published his analysis of steel. It was very natural to try the effect of sulphurous acid on iron, and therefore the fact might easily be discovered by various persons, without communication with each other. But that I had no desire to bury Mr. Higgins' book in oblivion, will be obvious from this circumstance, that I recommended the perusal of it several years ago to Sir H. Davy, and expressed a hope that the

author of so ingenious a work would still resume his chemical labours, which might be so useful to the science. I am happy to perceive that this expectation is now likely to be fulfilled.

Mr. Higgins says that he was as well acquainted with metallic oxides when he published his Comparative View as he is at present. Now this may be the case, as I do not know how Mr. Higgins has spent his time during the last fifteen years. But in that case he has a great deal to learn before he can bring his promised System to be an adequate representation of the present state of the science.

With respect to the combination between Mr. Dalton, myself, and some others unknown, to deprive Mr. Higgins of his discoveries, at which he hints in his Appendix, I cannot avoid expressing my astonishment that such a notion could have been taken up by any man of common sense. Had the theory been claimed by the late Mr. Cavendish, or by any man of rank and influence, such an allegation might have had some plausibility. But Mr. Dalton is neither a man of rank nor influence. He is a Quaker, who has supported himself for many years in Manchester by teaching mathematics and arithmetic, and by giving occasional lectures on chemistry. I do not believe that his income amounts to one third of that of Mr. Higgins. Of course his influence ought, one would think, to be proportional. He is, indeed, highly esteemed by all that know him, for his suavity of manners, integrity of conduct, openness, ingenuity, and invention. My first knowledge of him was in consequence of an attack (rather rude I thought at the time) which he made upon me in Nicholson's Journal. I never saw him till 1804, and I have only been twice with him since ; once at Edinburgh, where he gave a course of five lectures in my class-room ; and once in Manchester, when I spent a considerable portion of two days in his company. I wrote the note which has occasioned all this discussion because I thought Sir H. Davy treated Mr. Dalton harshly and unjustly in the notes to which I have formerly alluded. I was not ignorant of the reasons which prepossessed Davy against him, and his notes struck me as something like an attempt to crush him by the superior weight of his own name and situation. I own I felt rather hurt at this, which appeared to be somewhat allied to oppression ; and I thought myself entitled to notice it, because Sir H. Davy elevated myself at Mr. Dalton's expense. I ascribed Davy's notice of Mr. Higgins being the author of the atomic theory to the same cause that induced him to write the first note ; and as the opinion had been noticed by Berzelius, I thought it in some measure incumbent on me to notice it ; and, as far as I could, to set Berzelius right on the subject ; never dreaming that I was to be attacked by the formidable pen of Mr. Nash, and the pathetic insinuations of Mr. Higgins. But notwithstanding all that has happened, I do not repent of the steps I have taken. My object was to support a man of merit, who I thought was unjustly used. I do not wish to quarrel with any man, far less with a man whose genius and abilities I esteem. But if the alternative be held

out to me, either to quarrel or to act a dishonest and sneaking part, I never hesitate in any case, whatever the ultimate consequence may be to my literary reputation or my worldly interest, to embrace the first alternative, and spurn the second. This is not the first time that I have stepped forward from similar motives; and I might have occupied a very different situation from the one I fill at present if I had adopted the time-serving policy of some of my acquaintances.

Article IX.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, the 26th of May, a paper, by Sir Everard Home, Bart. was read, On the Effect of different Injuries in the Brain upon Sensation. The attempts to determine the functions of the different parts of the brain not having been attended with success, Sir Everard conceives that it would promote the advancement of physiology if medical men were to collect together, and arrange the effects produced by different diseases or injuries of the brain. The present paper contains the result of his own practice. It is divided into ten sections. 1. On the effect of water accumulated in the ventricles. Water accumulated in the ventricles, even to the amount of $6\frac{1}{2}$ ale pints, does not destroy the faculties, provided the bones of the cranium be not united, and the head enlarge according to the accumulation. A curious case was related of a young man whose head had increased enormously, and who retained his faculties entire, except some inconveniences from the size and weight of the head. He was in his nineteenth year, and the head was 33 inches in circumference. When the bones of the cranium, being united, prevent the head from enlarging, the accumulation of water in the ventricles destroys the faculties, and produces idiotism and death. 2. On the effects of concussion. It occasions nausea and vomiting, giddiness, and apoplectic fits, which return at intervals for some time. 3. On the enlargement of the blood-vessels of the brain. It occasions violent headaches, watchfulness, and disorders of the bowels. The beating of the arteries of the brain has been supposed essential to the exertion of the senses; but John Hunter retained his senses after the heart had apparently ceased to beat. 4. On the extravasation of blood. It produces similar effects as the accumulation of water; coma, nausea, apoplexy. 5. On the effects of the formation of pus. It occasions melancholy, lowness of spirits, and mania, with incessant talking. 6. On the effects of external pressure. The depression of the scull occasions loss of memory, the incapacity of using the proper

conversation, &c. all which disappear when the cause is removed. 7. Internal pressure from tubercles produces similar effects. 8. Hydatids in the brain occasion bowel complaints, and a comatose state of the rectum and bladder. 9. Wounds in the brain occasion no symptom whatever, nor the destruction of any of the faculties. When a fungous excrescence of the brain takes place through a hole in the scull, the esophagus becomes so sensible as to prevent swallowing solids, from the pain which they occasion. 10. Injuries of the spinal marrow in the neck occasion paralysis of all the parts of the body below the injury.

On Thursday, the 9th of June, a paper by Sir Humphry Davy was read, containing additional observations on iodine. The paper consisted of five parts. Part first contained additional observations on the triple compounds formed by dissolving iodine in solutions of alkalies and alkaline earths. The author had already ascertained that when iodine is dissolved in a solution of potash two different substances are formed; the first a compound of oxygen, iodine, and potassium; the second a compound of iodine and potassium. The first of these compounds, which is analogous to hyperoxymuriate of potash, is nearly insoluble in water. To obtain it pure, it must be boiled repeatedly in alcohol, which dissolves the iode of potassium, but leaves the triple compound, to which Davy gives the name of *oxiode of potassium.* This substance dissolves in sulphuric, phosphoric, acetic, oxalic, and indeed in all the acids tried, without undergoing decomposition. At a red heat it gives out oxygen gas, and sulphuric acid poured upon the remainder expels iodine, while sulphate of potash is formed. From two analyses, but both upon a very small scale, Davy concludes that this salt is precisely similar in its composition to hyperoxymuriate of potash, only that iodine comes in place of chlorine. Oxiode of sodium is analogous in its properties to oxiode of potassium. Oxiode of barium is nearly insoluble in water. The author did not succeed in his attempts to obtain a separate compound of iodine and oxygen.

Part second consisted in experiments on hydrionic acid. This acid dissolves iodine, and forms an orange coloured solution. It absorbs oxygen from the atmosphere. The compounds which it forms with bases are very similar to the analogous muriates. When oxygen is present they are decomposed by heat, iodine being disengaged; and potash, lime, or whatever constituted the basis, left behind.

On Thursday, the 16th of June, the remainder of Sir Humphry Davy's paper on iodine was read. In the third part the author gave an account of his experiments on the combination of iodine and chlorine. When iodine is heated in a dry vessel filled with chlorine gas it absorbs about one third of its weight of that gas. The compound formed dissolves in water, forming a very acid liquid, which in the author's opinion owes its acidity to the properties of this chloride of iodine, which of course he considers as an acid. When agitated in chlorine gas, this substance becomes colourless. It has

the property of dissolving a good deal of iodine, and acquires in consequence a dark colour. The author conceives this chloride to be a compound of one atom of chlorine and one atom of iodine. In the fourth part the author gave an account of the action of iodine and several of the compound gases. When sublimed repeatedly in sulphureted hydrogen gas, a red liquid is formed, which is a compound of the gas and iodine. When dissolved in water, sulphur is deposited, and hydrionic acid formed. When iodine is heated in olefiant gas, a very small quantity of a colourless liquid, having an acrid taste, is formed. On nitrous gas and carbonic oxide it produces no change. The fifth part of the paper consisted in experiments on different marine productions, to determine whether they contain iodine. In the sea water of the Mediterranean he did not find any traces of it. He found traces of it, but in very minute quantity, in some fuci and ulvæ, but not in any of the confervæ or sponges which he examined. In the author's opinion it exists in the ashes of these plants in the state of oxiode of sodium. The most delicate test of iodine he found the property which its salts had of discolouring polished silver. A very minute quantity of it in a salt makes the salt capable of forming a red solution in sulphuric acid.

At the same meeting a paper by Dr. Kidd, Professor of Chemistry at Oxford, on the formation of nitre on walls, was read. This salt appears spontaneously on the walls of the Laboratory at Oxford, and Dr. Kidd relates the result of his observations on its appearance, continued for about a year. The wall is composed of limestone, containing 96 per cent. of carbonate of lime, and four per cent. of oxide of iron, clay, sand, and a trace of magnesia. The nitre only appears on this limestone. Frost increased the rapidity of the formation; but an intense cold seemed to put a stop to the process. Moisture seemed injurious to the process. The free excess of air was requisite for the formation of the salt in the usual quantity; though a portion was formed on a part of the wall separated from the external air by means of a glass-case; but after a certain time this formation ceased. Pure carbonate of lime spread upon the glass-case did not yield any saline efflorescence. The saltpetre was nearly pure. It did not contain above half per cent. of lime, and gave very minute traces of sulphuric and muriatic acids.

On Thursday, the 23d of June, a paper by Sir Humphry Davy was read, on the combustion of diamond in oxygen gas. These experiments were made at Florence by means of the large burning-glass, which was used in the first combustions of diamond at the expense of the Archduke Leopold. They were afterwards continued at Rome. The oxygen gas was prepared from hyperoxymuriate of potash, and introduced into dry glass globes, fitted with stop-cocks, and capable of holding from 14 to 40 cubic inches of gas. The diamond was put into a small platinum capsule full of holes, and attached to the stop-cock. The absorption of gas was determined by means of a small glass tube properly fitted to the stop-cock,

graduated, and plunged in mercury. When the diamond was once kindled by the burning-glass it continued to burn for some time, though the burning-glass was withdrawn, and even melted a platinum wire by which it was attached to the tray. No moisture was produced by the combustion, and the diminution of the bulk of gas was scarcely sensible. Nothing was produced but carbonic acid gas, possessing all the properties of common carbonic acid gas. When plumbago was burned, some moisture was produced, and the bulk of the gas was sensibly diminished, indicating the presence of a small portion of hydrogen as a constituent of this substance. When two grains of plumbago were burnt the absorption amounted to the bulk of 96 grains of mercury; but as some oxide of iron was left, the whole of this absorption cannot be ascribed to the formation of water. Charcoal from turpentine burnt all away, without leaving any residue, and produced a greater quantity of water, and a greater diminution of the gas, than plumbago. Charcoal from alcohol, formed during the making of ether, left a small white residue, owing probably to the impurity of the sulphuric acid employed in the process. It formed more water, and occasioned a greater absorption. Charcoal from oak left a white ash, consisting chiefly of carbonate of lime. It formed the greatest quantity of water, and occasioned the greatest absorption. These experiments lead to the conclusion that diamond is pure carbon, and that the other substances contain a little hydrogen in a state of chemical combination. Sir H. Davy conceives, with Mr. Tennant, that the difference between charcoal and diamond depends chiefly on the crystallized state of the latter.

At the same meeting a paper by Smithson Tennant, Esq. was read, on an easier mode of procuring potassium than the process of Gay-Lussac and Thenard. The method is this: A piece of gun-barrel, about 18 inches long, shut at one end, is covered with a lute composed of raw and baked Stourbridge clay. A piece of gun-barrel, about nine inches long, open at both ends, is made to slip into the upper end of the first gun-barrel. The lower end of this piece has a narrow opening. It may be fixed on with sealing-wax, or any common lute. Over the open end a third tube, or cap of tin plate, is luted, having a perforated cork, through which there passes a bent tube of safety. A mixture of caustic potash and iron turnings is put into the bottom of this gun-barrel, and it is then heated violently for about an hour in a common smith's forge. The potassium is found sublimed pure in the interior iron tube.

At the same meeting a paper by Sir Everard Home, Bart. was read, giving an account of the skeleton of a fossil animal related to the class of fishes lately discovered on the south coast of England. This specimen is in Mr. Bullock's museum. It was discovered in a limestone rock situated on the west of Lyme. The scull was found in 1812, and the other parts some months after. The author shows that it must have been a swimming animal, from the articulations of the vertebræ. But it differs in its structure from all

known genera of fishes, and the author conceives that it is related to some of those intermediate animals found upon the coast of New South Wales.

LINNÆAN SOCIETY.

On Tuesday the 7th June, a paper was read on the long sea-worm, the Gordius Maximus of Montague, by the Rev. Hugh Davies.

At the same meeting there was read a description of a fossil alcyonium from the chalk strata near Lewis, by Gideon Mantell, Esq.

On Tuesday June 21st, a description of the cajeput oil tree, by Dr. Roxburgh, was read. In 1797 it was brought from the Maldives to the Botanic Garden at Calcutta, where it throve very well, multiplied considerably, and produced blossoms and perfect fruit. It was from these trees, about 13 years of age, that Dr. Roxburgh's description was taken. The stem was slender, considering the height of the young tree, which was about 20 feet; the bark smooth and peeling off like that of the birch; the twigs pendulous like those of the weeping willow; the leaves lanceolate and smooth. They yield an aromatic oil. He considers it as a new species, to which he gives the name of melaleuca cajeputi.

At the same meeting, a paper by Dr. Leach was read, on the class of insects called myriapoda; in which the animals composing it were distributed into two orders, viz:

1. *Chilognatha.* Maxillæ, none; palpi, indistinct; labrum, simple.

2. *Syngnatha.* Maxillæ, two, distinct, connected at their base; palpi, four; maxillary ones filiform, simple; labial ones terminated by a little hook.

These orders were considered as constituting distinct classes by Fabricius; but as they agree in anatomical structure they cannot be viewed in that light. Latreille placed them with the *Arachnides*, on which class a memoir by the same gentleman was read, in which the following arrangement was proposed.

Subclass. 1. *Octopoda*, feet eight. This division contains the following orders, the character being founded on the structure and divisions of the body, viz: 1. *Podosoma.* 2. *Polymerosoma.* 3. *Duomerosoma.* 4. *Monomerosoma.*

Subclass. 2. *Hexapoda*, feet six. This subclass consists of two orders, which are formed from the situation of the mouth; viz: 1. *Cephalostoma.* 2. *Notostoma*, which latter order contains the genus *Nycteribia* of Latreille, *Ptheridium* of Hermann.

In the paper on the Arachnides, he related a curious observation by Sir Joseph Banks. This gentleman observed one day a spider running across the table with a fly in its mouth; surprised at this sight, he caught it, and found it a common web-spinning spider; but having only three feet instead of eight, the loss of its limbs had prevented it from spinning its web, and obliged it to

1

alter its economy. After keeping it some time, the five legs began to sprout out, but they only attained half the length of the old ones, and were not therefore of much use. But they enabled it to spin a web on which it usually sat. Every time the animal cast its skin the feet became longer.

The Society adjourned till the first of November.

WERNERIAN SOCIETY.

At the meeting on the 16th of April, the Secretary read a communication from the Rev. Dr. Fleming, on the species of *mus* found in Scotland. The original genus mus has been subdivided into several genera, *myorus, arvicola,* and *mus.* The first genus includes only the dormouse, which was observed in Scotland by the late Dr. Walker, but is rare. Of the genus arvicola, Mr. Fleming mentioned three species, *agrestis, terrestris,* and *amphibius.* Of the restricted genus, mus, he enumerated six species, viz: the common mouse; the field mouse; the harvest mouse; the black rat, M. *rattus*; the brown or Norway rat, M. *decumanus*; and the *mus niger,* which Mr. Fleming procured in Linlithgowshire, and which he considers as a species hitherto nondescript.

At the same meeting, the Secretary read a communication from Mr. W. Bullock, giving an account of some rare birds observed by him among the Orkney islands, in the summer of 1812. He found in Hoy all the four species of eagle, generally accounted British, viz: the golden, the ancreous, the ring-tailed, and the sea eagle. In North Ronaldsha he observed the large snowy owl; and near Passa Westra the great auk. The first of these had not before been ascertained to be British; the latter has scarcely been seen on our shores for the last 50 years. Specimens of both are now preserved in his museum.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(Continued from vol. iii. p. 467.)

A portable Barometer of a new Construction. By M. Gay-Lussac.

This is a syphon barometer. What distinguishes it from every other known at present, is that it has neither stop-cock, screw, nor piston. The shorter branch is shut at its extremity: but two or three centimetres below this extremity there is a capillary hole, sufficient for the free admission of the air, but too small to allow the mercury to escape, even when it passes over the opening.

This branch is united to the larger branch of the syphon by a tube, the interior diameter of which is about a millimetre, and its length above the curvature two or three decimetres. This disposition has the advantage, that if air enters into the curvature of the barometer during the carriage, the mercury will expel it when the

instrument is reversed. This would not happen if the tube were wider. This construction is so simple, that it is conceived the preceding description will be sufficient to give an exact idea of it without a figure.

Count Rumford has read to the Class the description of a thermometer destined to measure the specific heat of solids and liquids. But this memoir has not been communicated to us.

Books presented to the Class by Members and Correspondents during the course of 1813.

Exercises on the Integral Calculus; supplement to the first part by M. le Chevalier Legendre.

The author in this supplement proposes to make known a new and extensive class of definite integrals, which may be expressed in part by eliptic functions, in part by the arcs of circles and by logarithms. These applications, joined to all those that he has given in the first part, demonstrate more and more the necessity of admitting eliptical functions into the integral calculus to the same rank as the arcs of circles and logarithms. This will require pretty extensive tables of the functions of the first and second species. The author has already shown shortly, how such tables may be constructed; he now promises to give the formulas proper for shortening the labour, and furnishing new means of executing it.

It is obvious that a work of this kind, which is in fact merely a collection of formulas, cannot be analysed. We shall therefore merely say, that the author for the sake of greater clearness, has arranged all the integrals under 16 cases, which form as many particular tables, in which we may observe the whole results by a glance of the eye, and find those for which we have occasion : that he has often varied the solutions by showing the different ways by which we may arrive at the same expression; and that he has marked out some particular cases, which from their singularity and difficulty seem to him to deserve all the attention of mathematicians.

Reflections on the Metaphysics of the Infinitesimal Calculus, by M. Carnot. Second Edition.

In speaking of the first edition of this work M. Lacroix gave this testimony in its favour, that *the metaphysics of the calculus was presented in a new, ingenious, and concise manner.* Nothing more clear, or more satisfactory has been written in favour of the algorithm of Leibnitz. But notwithstanding the preference which he gives to this system, the author passes in review the different points of view under which this theory may be examined. He answers the different objections which have been started against it. *It is always the method of exhaustion of the ancients more or less simplified, more or less happily appropriated to the requisites of the calculus and reduced to a regular algorithm.* The same principles enable him to explain the principles of the calculus of variations of the immortal Lagrange. This new edition is enriched with an ad-

ditional note in which the author gives new developments of his theory of negative quantities.

Observations on the great Comet of 1807, *with a Supplement to the Aphroditographic Fragments.* By M. Schrœter; Gottingen, 1811. In German.

The author has attached himself to the physical consideration of this comet. The objects of his observations are the particular phenomena which it offered, its nucleus, its nebulosity, the variations in its tail, its direction, and its proper light.

In the new observations of Venus will be remarked the attempts made by the author to determine the time of the rotation of Venus, by phenomena which he observed in the horns of her crescent. By a multitude of calculations and comparisons of which it is impossible to present the details, he comes to this result. The time of rotation is 23 hours 21′, 7·977″. This confirms the old determination of Cassini. The author suspects that this planet has a peculiar phosphorescent light of its own. He thinks this is the case also with Mercury, and perhaps with all the planets in our system.

Theory of Analytical Functions; by M. Lagrange. A new edition, revised and augmented by the author.

This second edition has several advantages over the first edition which appeared in 1797. It is more correct; it is better arranged, and is divided into chapters, which was not the case at first, as the work had been originally printed as fast as written. There are also several additions, the principal of which are in the 14th chapter of the second part, and the fifth chapter of the third part. The author presented it to the Class 47 days before his death.

An Abridgement of Astronomy, by M. Delambre. This work is an abstract of the lectures which the author gave in the Imperial College of France, the complete collection of which in three volumes 4to. will appear early in 1814. It contains all the astronomical calculations reduced into formulas, and presented in the order which appeared the most natural.

The Sixth and last but one Part of Researches on the Monuments of indigenous Inhabitants of the New Continent, by M. de Humboldt.

This part is terminated by a memoir of M. Visconti on the relations which subsist between the American monuments and those of the ancient continent.

Memoir on several Physical Properties newly discovered in the Molecules of Light; read at the public sitting of 1813, by M. Biot.

Fourth edition of the Systeme du Monde; by M. Laplace.

Description of the Harbour near Lindou on the Lake of Constance.—Second Volume of the Construction of Bridges and other Buildings on Water, with 46 *Plates.* These two works are by M. Wiebeking.

Tables of the Divisors of the Second Million, or more exactly from 1,020,000 *to* 2,028,000, *with the Prime Numbers that occur among them.* By M. Burckhardt; Paris, at Madame Courcier's.

These tables form a very useful supplement to those of M. Chernac, which have the same form, and are sold in the same place. This task which appears immense, and which must have been particularly disagreeable on account of its uniformity, was performed at spare hours, and facilitated by simple and ingenious methods which rendered it at once shorter and more accurate. After the example of Lambert the author has contented himself with giving the smallest divisor of every number, from which the greatest may be immediately deduced by an easy division. To have all the other divisions it is sufficient to seek for the quotient in the table of M. Chernac.

After the actual division which enables us to find by repeated essays the smallest divisor of each number, the first method that presents itself is that of constructing by simple additions tables of the multiples of all the prime numbers, stopping at the multiple which would pass the limits of the projected table. But this labour would be merely preparatory; it would remain to transcribe in another order the whole table, and there would be a great many useless numbers, because it has been agreed upon to suppress in all these sorts of tables all the numbers divisible by 2, 3, and 5, because they may be known by simple inspection. By the arrangement which the author has contrived to give to his preparatory tables, the divisors 7 and 11, which occur more frequently than any of the others, are similarly placed in all the pages. A copper plate engraved was sufficient for all this part of the table. M. Burckhardt successively employed, and has shortly explained in his preface, different contrivances, some of which were already known, while others belong to himself exclusively, and had escaped Euler who had condescended to write a long memoir on the composition of these tables. The goodness of such a work depends upon the care with which it has been printed. M. Daussy divided with the author the labour of revising the proof sheets. M. Daussy is known for a patient and skilful calculator; we are indebted to him for the elements of different comets. We may therefore expect to find these tables as exact as they can be made by the labour of man.

ARTICLE X.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Singular Disease.*

IN the country of the Nogays, a tribe of Tartars dwelling between the Black Sea and the Caspian, on the south side of the river Kuma, there still exists a very singular disease, which is mentioned by Herodotus, and several of the other ancient Greek writers.

Herodotus informs us that when the Scythians were inhabitants of Asia they advanced towards Egypt; but were prevailed upon by Psammetichus, King of that country, to desist. On their return through Syria they plundered the temple of Urania, in the city of Askalon. In consequence of this, the goddess sent a *feminine disease* among them.

Reineggs is the first modern writer who mentions the present existence of this disease among the Nogays, who are at present subjects of the Russian empire. Count Potocki, when travelling along the Kuma, in 1798, met with an old man who had this disease. He informs us that such persons are called Coss; and that the disease is not unknown in Turkey, where those subject to it have received the same appellation. The disease, as far as it has been described by Reineggs and Potocki, is distinguished by the following symptoms. It only attacks old persons. The skin grows wrinkled, the beard falls off, and the person assumes completely the appearance of a woman. He becomes incapable of propagating his species, and his sentiments and actions lose their masculine character. In this state he is obliged to shun the company of men, and to associate with women, whom he perfectly resembles. The disease is now rare.

II. *The Caucasus.*

The Caucasus is a well known range of mountains between the Black Sea and the Caspian. According to the account of Julius Von Klaproth, the two highest summits of this range are known by the names of Elbrus and Niquirwari. The former he considers as larger and higher than Mount Blanc, and the latter as fully equal to that mountain in size. Klaproth's account of the structure of these mountains is very remarkable; but I am not certain that we can rely upon the names of the rocks in the English translation, and I have not had an opportunity of examining the original. Klaproth, who is the son of the most celebrated chemical analyst of the present age, ought to be acquainted with the technical names of rocks; but the English translation bears very evident marks that the translator was at least unacquainted with the English names commonly given to minerals.

The external range consists of limestone. As we advance towards the centre, rocks of clay slate make their appearance; at first brown, but as we proceed they become blue, and acquire a greater degree of hardness. The centre consists of red basaltic porphyry and clay porphyry, intermixed more or less with glassy felspar, and a small portion of mica: for the chief mass of this porphyry, he observes, seems, in a mineralogical point of view, to be a distinct mineral from basalt; it is nevertheless very nearly allied to it, and composes one formation with the basaltic porphyry proper, into the principal matter of which it not unfrequently changes. Where the principal mass ceases, a species of hornblende, which appears some-

times in small, at others in very diminutive reddish brown and brownish red crystals, specifically different from the basaltic, is exclusively peculiar to this stone.

III. *New Species of Honey.*

In the kingdom of Imerethi, which constitutes a part of Georgia, and lies at the east end of the Black Sea, at present subject to Russia, there is found a kind of wild honey, which is called by the inhabitants stone-honey, because it is quite solid, brittle, and not viscous. It has a pleasant and aromatic flavour, and is found in the clefts of the rocks. Honey and wax form one mass, and are as hard as sugar-candy. The cakes are commonly white, but turn yellow with age, and will keep for a long time. The Imerethians carry it with them in their pockets.

IV. *Supposed Urinary Calculi.*

Some weeks ago I received from a surgical friend a paper containing some specimens of stones, which, he informed me by letter, had been passed by a boy of about ten years of age. On opening the packet, I was very much surprised at the appearance of these calculi. They were about twenty in number, the largest about the size of a millet seed, and the smallest not larger than grains of sand ; the shape was irregular ; some of them were rounded, some had sharp edges ; they were mostly all transparent, had a vitreous fracture, could not be scratched by a knife, and readily melted with potash into a glass. In short, they were fragments of quartz stones, which the boy had picked up, and which he pretended to have passed, merely to deceive his parents, for some purpose or other, which may easily be conceived.

V. *Substance extracted from the Vagina of an old Woman.*

From the same gentleman I received a specimen of a substance which had been extracted from the vagina of an old woman. It is of a yellowish white colour, smooth upon the outer surface, has the hardness of bone, and easily breaks. When heated it gives out the smell of burning feathers, and becomes black ; but if it be kept for some time red-hot, it becomes quite white, and has the aspect of burnt bone. It dissolves slowly, and without effervescence in muriatic acid, from which it may be again precipitated by caustic ammonia. These properties leave no doubt that it consists of phosphate of lime, cemented together by an animal substance, which is probably of the nature of mucus. When a portion of this matter is kept for some days in a weak acid, almost the whole dissolves, and the undissolved portion is in a state of white flocks.

VI. *Prince Rupert's Drops.*

Dr. Brewster has discovered the existence of three cleavages in the glass tears, well known by the name of Prince Rupert's drops,

two of them appear to cross the axis of the drop, forming with each other angles of about 80 and 100. These observations show that the substance is crystallized.

VII. *Remarkable Petrifaction.*

Soemering has published lately, at Munich, a paper upon a fossil skeleton eight inches in length found imbedded in calcareous rock near Aichstadt. The paper is accompanied by three figures, two of the fossil as it now exists, and in the third the dislocated bones are arranged so as to form a complete skeleton.

He had the specimen itself in his possession, and determines it to be the osseous remains of an animal of the bat tribe, approaching to the *pteropus pusillus* of Lechenault. He gives it the name of *ornithocephalus.*

Cuvier had never seen the specimen itself, but made his observations on a figure of it published in 1784 by the late Collini, keeper of the Electoral Museum at Manheim. He decides that it belongs to a new genus in the family of lizards.

Soemering occupies a considerable part of his paper in refuting the conclusions contained in Cuvier's dissertation on this osteolite. He finds the number of cervical vertebræ to be seven, a number which is constant in the mammalia. In Collini's figure the number is not correctly represented. The number of the phalanges digitorum appears greater than it really is, because the epiphyses are separate from the middle part of the bone, as they are in the humerus, femur, &c. By this apparent number of the phalanges the skeletons of the paws in Collini's figure is likened to that of the paws of lizards; there is a large cavity anterior to the orbit which in mammalia is filled with the corniculated bones of the nose; the defect of these in the fossil may arise from their cartilaginous state, owing to the immaturity of the individual, which is seen to have been young by the unconnection of the epiphyses just mentioned. A separate bone, to which the under jaw is articulated, is represented in Collini's figure, and is characteristic of lizards. Upon a careful examination of the fossil, Soemering could perceive no such bone; the under jaw appeared to have been articulated as in mammalia; the intimate substance of the bones he found to resemble that of the bones of mammalia, and not that of the bones of lizards. He gives various other characters, and a detail of the comparison he instituted between the fossil and a number of skeletons of known animals, our cotemporaries in the present creation. These induce him to adopt the conclusion above mentioned, and to reject Cuvier's.

The museum at Munich contains some other ancient animals found in the calcareous strata near Aichstadt. Amongst these are a crab and an ichthyolite; the former nearly related to the molucca crab (*monoculus polyphemus*); the latter to *stromateus*, a genus of fish, all the known species of which are inhabitants of the equatorial seas.　　　　　　　　　　　　　　　　　　　　　　　　　C.

Article XI.

New Patents.

Lewis Gompertz, Kennington Oval, Surrey; for sundry improvements in carriages (and substitutes for wheels for carriages) and other machines. April 27, 1814.

David Grant, Pickett Street, Strand, London; for a pump or apparatus for drawing off soda water and other liquids impregnated with fixed air. April 27, 1814.

John Barnard Logier, Sacville Street, Dublin; for an apparatus for facilitating the acquirement of proper execution on the piano-forte. April 28, 1814.

Joseph Price, of Gateshead, in the county of Durham, glassmaker; for several new methods of making glass. May 5, 1814.

John Vancouver Jekenham, Middlesex, Esq.; for a method of painting walls of apartments and other surfaces, by the preparation, use, and application, of certain materials for that purpose. May 17, 1814.

Thomas Abice Pickering, Hackney Terrace; for a method for preventing the loss of parcels (containing bank notes, bank post bills, country bankers' or other notes or bills payable to bearer in London or in the country) by coaches, so frequently recurring. May 21, 1814.

Article XII.

Scientific Books in hand, or in the Press.

The Second Volume of the Transactions of the Geological Society will be ready for delivery to the Members early in July.

Mr. Arthur Aikin, Secretary to the Geological Society, is about to publish a Manual of Mineralogy.

Dr. J. R. Johnson, of Bristol, F. L. S. &c. intends shortly to prepare for Press a Treatise on the Medicinal Leech, to which will be subjoined an engraving illustrative of its interesting and highly singular structure.

Dr. Jameson, of Cheltenham, will shortly publish a Tract on Cheltenham Waters transferred to Reservoirs, and at the Fountain Head.

Capt. Flinder's Voyage to Terra Australis will be published in a few days, by order of the Lords of the Admiralty. It will be illustrated by a large Volume of Charts, Head Lands, and Botanical Subjects.

ARTICLE XIII.

METEOROLOGICAL TABLE.

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1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
5th Mo.										
May 11	N E	30·42	30·28	30·350	57	34	45·5	—		
12	N E	30·28	30·08	30·180	58	40	49·0	—	—	☽
13	N	30·08	29·97	30·025	52	42	47·0	·10	·21	
14	N	29·97	29·93	29·950	55	42	48·5	—		
15	N E	29·94	29·89	29·915	59	34	46·5	—		
16	S E	30·06	29·94	30·000	62	40	51·0	—		
17	N E	30·09	30·06	30·075	68	43	55·5	—		
18	E	30·06	30·04	30·050	68	40	54·0	·40		
19	S E	30·04	29·85	29·945	70	41	55·5	—		●
20	S E	29·85	29·70	29·775	62	42	52·0	—		
21	S E	29·70	29·48	29·590	60	36	48·0	—		
22	S E				52	39	45·5	—		
23	N W				54	41	47·5	—	—	
24	N W				46	41	43·5	—	—	
25	N W	29.87	29·57	29·720	58	31	44·5	—	—	
26	N E	29·87	29·83	29·850	63	33	48·0	—		☾
27	N E	29·83	29·58	29·705	66	39	52·5	—		
28	N E	29·65	29·56	29·605	70	39	54·5	—	1·75	
29	N W	29·96	29·65	29·805	70	42	56·0	—		
30	N W	30·01	29·96	29·985	63	48	55·5	—		
31	S W	30·01	29·94	29·975	67	46	56·5	1·00		
6th Mo.										
June 1	S E	29·94	29 79	29·865	58	47	52·5	—	—	
2	E	29·80	29·73	29·775	53	46	49·5	—	—	
3	E	29·75	29·65	29·700	55	50	52·5	—	—	○
4	N E	29·90	29·75	29·825	55	45	50·0	—	·77	
5	N E	30·07	29·90	29·985	54	44	49·0	—	—	
6	N E	30·07	30·00	30·035	55	43	49·0	—	—	
7	N E	30·00	29·91	29·955	58	40	49·0	—	—	
8	N E	29·97	29·89	29·930	67	41	54·0	—		
9	N E	30·03	29·97	30·000	70	36	53·0	·55		
		30·42	29·48	29·910	70	31	50·50	2·05	3·73	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fifth Month.—13, 14. Cloudy: much wind. 15. Cloudy and windy: the *Cumulostratus* prevails. 16, 17. Misty mornings. 18. A breeze, a. m. from N. E. with *Cirrus, Cirrocumulus,* and *Cirrostratus* clouds. 19. A clear morning. 24. A very wet day and night. 25. A *Stratus* on the low grounds at night. 26. Ice on the pools of standing water this morning to the thickness of $\frac{1}{16}$ of an inch.

Sixth Month.—2. A slight shower last evening: and this morning, small rain. 8. Much wind: cloudy.

RESULTS.

Prevailing Winds Easterly.

Barometer: Greatest height ....30·42 inches;

Least (observed) ....29·48 inches;

Mean of twenty-seven days ..29·910 inches;

Thermometer: Greatest height ....70°

Least ....31°

Mean of the period ....50·50°

Evaporation, 2·05 inches.   Rain, 2·73 inches.

The minimum of the barometer for the period is somewhat uncertain, from the loss of three days' observations about the time of its occurrence: the depression was followed by frost, as is frequently the case when much rain has fallen. The first of the present month was a contrast to the same of last year, when the thermometer rose to 85°; yet it is observable that the same low temperature occurred then also four days afterwards. On the whole this period has been more changeable than we should have expected with an easterly current prevailing, and full 6° colder than the corresponding one of 1813, in which westerly winds predominated. Birds of song are remarked to have been less heard than usual; a circumstance perhaps ascribable to their number having been reduced by the severity of the winter.

TOTTENHAM,   L. HOWARD.
Sixth Month, 21, 1814.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1814.

ARTICLE I.

Biographical Account of Mr. Williams, the Mineralogist.[*]
By Patrick Neill, Esq. F.R.S.E.

MR. WILLIAMS was the son of a clergyman in Glamorgan-shire, South Wales, and was born about the year 1730. While a boy he resided a good deal at the copper-mines of Anglesea, and thus very early acquired a taste for mining pursuits. Having reached manhood, he travelled into Scotland, and was fortunate enough to be employed by the Commissioners for Forfeited Estates in the survey of some of the extensive Highland domains placed in their hands. In this employment he spent a considerable number of years. It was in the course of this survey that his attention was particularly attracted by the *vitrified forts* of the north of Scotland; of which he afterwards published an account.

About the year 1770 he took a lease of a coal-work on the banks of the river Brora, in Sutherland. The seam then worked proved to be of very limited extent, and the coal was so sulphureous, that when taken on ship-board, to be conveyed to the towns situated on the Moray and Cromarty Friths, it was apt spontaneously to take fire. This speculation, therefore, proved unsuccessful;[†] and was abandoned in 1774.

After this Mr. Williams was successively engaged in the working

[*] From a note in Mr. Neill's translation of Daubuisson on Basalt; a book of which an analysis is given in the present number of the *Annals of Philosophy*.

[†] The coal on the banks of the Brora has, of late years, been again tried, by order of the Noble and enlightened proprietor, Earl Gower, and promises fair to be useful to the North of Scotland. In a new pit, opened in 1811, at the depth of somewhat more than 200 feet, a bed of " hard caking coal," three feet three inches thick, was found; and almost immediately below it, a bed of " hard splent coal," one foot four inches thick.—*Sutherland Report*, 1812.

of the lead-mines at Wanlockhead; and of the silver-mines at Silver-Hills, West Linton. In these operations he embarked the greater part of the capital he had previously acquired ; and unfortunately neither of the concerns proved successful. He afterwards for some time superintended the coal-works at Blackburn, near West Calder; and in 1778 became overseer and factor at Gilmerton Colliery, belonging to Mr. Baird, of Newbyth. Here he remained above thirteen years. During this period his fame as a miner very greatly increased ; so that he was consulted by most of the coal proprietors in Scotland, whenever difficulties presented themselves in the course of their mining operations. In this interval he brought out his publications. In 1777, under the auspices of Lord Kames and Dr. Black, he published a description of the vitrified forts in the north of Scotland, in a curious tract, entitled, " Letters from the Highlands;" * and twelve years afterwards he produced his large work on the mineral kingdom ;† which, it is believed, met with but little attention from the public at the time, but which certainly laid the foundation for no small share of posthumous fame to its author.

About the year 1791 he left Gilmerton, and became engaged, along with the late Dr. James Anderson, in conducting The Bee, a periodical work then published at Edinburgh, and which acquired considerable reputation.

He afterwards spent nearly two years in travelling through England with Count Zenobia, visiting all the great manufactures and mines of that country ; and he was at last induced to go to Italy with that nobleman. On his arrival he set on foot the working of limestone, coal, and ironstone, on some of the Count's estates near Verona. He was going on with these improvements. when he was unfortunately seized with a typhoid fever, which proved fatal in the end of the year 1797, when he had entered his sixty-eighth year.

I cannot close this short notice without remarking, that the merit of Williams, as an accurate observer, and original thinker, is perhaps greater than many mineralogists are aware. While he certainly had not even heard of the Wernerian doctrines, he published, in 1789, his opinion that " water has been the agent in the formation of the strata ; and that all the phenomena which we behold upon and within the superficies of our globe, have been produced by water." He holds, that " it was water that brought and poured the ingredients of all the mineral ores into the cavities of the veins, while those ingredients were in a fluid state;" and that " dykes " are cracks or fissures filled with heterogeneous matters by water ;—just the Wernerian doctrines of mineral veins, and of the veins called " dykes." Further, he observes, that

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* Letters from the Highlands of Scotland, addressed to G. C. M. (George Clerk Maxwell) Esq. By John Williams, Mineral Surveyor. 4to. Edin. 1777. Mr. Creech was the publisher, and has still some copies in his possession.

† The Natural History of the Mineral Kingdom. By John Williams, Mineral Surveyor, F. S. S. A. In two vols. 8vo. Edin. 1789.

" coals, and their concomitant strata," occur in basin-shaped coal-fields, which are unconnected with each other ; thus making a remarkable approach towards distinguishing the *Independent Coal Formation* of the justly celebrated Professor at Freyberg. The opinion that beds of *whinstone* (meaning chiefly greenstone and basalt) are *unerupted* lavas had gained ground in Scotland, being supported especially by the late ingenious Dr. Hutton : Mr. Williams, however, shows, that " basaltic strata spread as wide, and stretch as far in the longitudinal line of bearing, as any concomitant strata, being regularly placed among the others which form the solid superficies of our globe ;—they have therefore been formed in the same way as the other strata :—the columnar and glebous figures which they sometimes exhibit have happened from drying ;— and it would be strangely absurd to imagine that burning lava could come in contact with coal without destroying it." * Besides, this essay on the mineral kingdom contains much valuable practical information, particularly on the interesting subject of coal-mining ; while some unsupported speculations, concerning " the mutations of our globe," and similar topics, in which the author has chosen occasionally to indulge, can very easily be forgiven.

Article II.

On the Daltonian Theory of Definite Proportions in Chemical Combinations. By Thomas Thomson, M.D. F.R.S.

(*Continued from p. 18.*)

Though only a year has elapsed since the publication of my essay on the weight of the atoms of chemical bodies in the second volume of the *Annals of Philosophy*, p. 32, so much additional information has been thrown upon the subject, partly by Dr. Wollaston's table to be published in the Philosophical Transactions for 1814, and partly from the great number of new analyses contained in Professor Berzelius' truly valuable paper on the cause of chemical proportions inserted in the second and third volumes of the *Annals of Philosophy*, that I already conceive it to be proper to point out the alterations which must be made in the table of atoms given by me (*Annals of Philosophy*, vol. ii. p. 42,) in order to render it a fair representation of the present state of our knowledge. I have myself made a few experiments in order to elucidate some

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* Mineral Kingdom, vol. ii. pp. 48. &c.; also Introduction, p. xliii. where some spirited remarks are introduced on the Huttonian Theory, which had been then newly announced.

particular points, which I shall notice as I proceed. I shall give in
the form of a table the atoms whose weights seem to require alte-
ration, together with a few bodies respecting the weight of the atom
of which I differ in opinion from other chemists.

		Weight of an atom.
	Oxygen	1·000
	Hydrogen	0·132 [a]
	Azote	1·803 [b]
	Phosphorus	2·618 [c]
283	Boron	0·733 [d]
284	Chlorine	4·498 [e]
285	Iodine	11·160 [f]

[a] This weight is founded on the result of my own experiments to
determine the specific gravity of hydrogen. Biot's experiments
make the weight 0·133, a difference too inconsiderable to require
correction. I still consider water as a compound of one atom
hydrogen and one atom oxygen, for reasons formerly assigned.
Davy, Wollaston, Berzelius, consider water as composed of one
atom oxygen and two atoms hydrogen. This would reduce the
weight of an atom of hydrogen to 0·066.

[b] I have given my reasons for this change in the *Annals of Philo-
sophy*, vol. iii. p. 135. Dr. Wollaston makes it 1·754.

[c] Rose found phosphoric acid a compound of 100 phosphorus +
114·7 oxygen. I have repeated this experiment several times, and
have obtained results that approach sufficiently near to satisfy myself
that Rose's numbers are very near the truth. Davy informs us that
he found phosphorous acid a compound of 100 base + 77 oxygen.
Now 77 is to 114·7 very nearly as two to three. Hence I think we
can scarcely hesitate to consider phosphorous acid as a compound of
1 atom phosphorus + 2 atoms oxygen, and phosphoric acid of 1
atom phosphorus + 3 atoms oxygen. Hence the weight of an
atom of phosphorus given in the text.

[d] This number is the result of the experiments of Davy and
Berzelius (*Annals of Philosophy*, vol. iii. p. 56), and supposes
boracic acid to be a compound of 1 atom boron + 2 atoms oxygen.
This is analogous to the composition of carbonic acid, an analogy
likely to hold good on account of the close resemblance between
charcoal and boron.

[e] The reasons which induced me to choose this number for the
weight of an atom of chlorine have been assigned in the *Annals of
Philosophy*, vol. iv. p. 13.

[f] This is the number which results from the experiments of Davy.
I shall very soon give an account of my own experiments to deter-
mine the weight of an atom of iodine.

	Number of atoms.	Weight of an integrant particle.
Nitrous oxide	$1\ a + 1\ o$	2·803
Nitrous gas..................	$1\ a + 2\ o$	3·803
Nitrous acid	$1\ a + 3\ o$	4·803
Nitric acid..................	$1\ a + 5\ o$	6·803 [g]
Phosphorous acid	$1\ p + 2\ o$	4·618
Phosphoric acid	$1\ p + 3\ o$	5·618 [h]
Ammonia	$1\ h + 1\ a$	1·935 [i]
Hydrophosphorous gas	$4\ h + 1\ p$	3·146 [k]

	Weight of an atom.
Magnesium	1·577 [l]
286 Ammonium	1·149 [m]
Alumina	2·136 [n]

[g] The change introduced into the weight of an atom of azote occasioned the alteration in the representation of these four compounds of azote and oxygen, as explained in the *Annals of Philosophy*, vol. iii. p. 135.

[h] These changes are the consequence of the new weight adopted for the atom of phosphorus, founded on the experiments of Rose and Davy, as stated in a preceding note.

[i] The reason of this alteration was explained in the *Annals of Philosophy*, vol. iii. p. 135.

[k] According to this statement, the gas is composed by weight of 100 hydrogen and 495 phosphorus. Now Sir Humphry Davy found by analysis that it was composed of 100 hydrogen + 489·56 phosphorus. This differs little more than one per cent. from the tabular result. If we confide in the specific gravity of phosphureted hydrogen gas taken by Sir H. Davy, it is just half the weight of hydrophosphorous gas, and ought therefore to be a compound of 8 atoms hydrogen + 1 atom phosphorus.

[l] This is deduced from the analysis of sulphate of magnesia by Berzelius, which is probably more exact than the analysis of Dr. Henry, on which my number in the original table was founded.

I do not consider it as necessary to make any alteration in the weights of potassium, sodium, barytium, calcium, strontium. Berzelius' numbers deviate a little from mine ; but mine are founded on the most accurate analyses hitherto made. I think no hypothetical reasoning, however plausible, should be allowed to influence us in determining the weights of the atoms of bodies.

[m] I do not consider the reasoning of Gay-Lussac and Thenard respecting the amalgam of mercury by ammonia as entitled to confidence. I have explained the foundation on which this number was established in the *Annals of Philosophy*, vol. iv. p. 14.

[n] Founded on Berzelius' analysis of alum.

	Weight of an atom.
Silver	13·714 [o]
Iron	7·143 [p]
Nickel	7·305 [q]
Zinc	4·095 [r]
Bismuth	8·994 [s]
Antimony	11·249 [t]
Tellurium	4·027 [u]
Arsenic	6·000 [x]

[o] This is founded on the analysis of chloride of silver, which is, I think, susceptible of more accuracy than any of the other methods of estimating the quantity of oxygen with which silver combines. I conceive oxide of silver to be a compound of 100 metal + 7·291 oxygen.

[p] In my first table I stated the weight of an atom of iron 6·666, supposing the black oxide a compound of 100 iron + 30 oxygen, and the red of 100 iron + 45 oxygen. These numbers coincide with the experiments of Hassenfratz, and do not deviate much from the experiments of Berzelius, and from some of my own published some years ago in Nicholson's Journal. But a set of experiments which I made last winter to ascertain how much iron was taken up by a definite quantity of sulphuric acid, which I consider as more susceptible of precision, led to the result that black oxide of iron is a compound of 100 iron + 28 oxygen. As this agrees with the experiments of Gay-Lussac and of Dalton, I think it preferable to my preceding number, especially as it nearly agrees with my own preceding experiments on the composition of black oxide of iron.

[q] The number for nickel in my original table is altogether erroneous. I do not know how it crept into the table.

[r] This number is the result of my experiments on the composition of blende, published in the present number of the *Annals of Philosophy*, to which therefore I refer the reader. Berzelius considers zinc as capable of combining with two doses of oxygen; but I have not been able to form more than one oxide of this metal, which I therefore consider as a protoxide.

[s] Berzelius informs us (*Annals of Philosophy*, vol. iii. p. 356) that he has discovered the existence of a purple protoxide of bismuth, besides the common yellow oxide. If this prove correct, we must double the weight of an atom of bismuth.

[t] See my paper on the analysis of sulphuret of antimony published in the present number of the *Annals of Philosophy*.

[u] The number in the original table was erroneous in consequence of an arithmetical error.

[x] I have not room here to assign my reasons for refusing to admit Berzelius' new determination of the composition of the oxides of

	Weight of an atom.
Cobalt	7·326 [y]
Manganese	7·115 [z]
Molybdenum	6·013 [a]
Tungsten	12·121 [b]
Cerium	11·487 [c]
287 Chromium	4·720 [d]

arsenic (*Annals of Philosophy.* vol. iii. p. 93); but I shall insert a short disquisition on the subject in the next number of this work.

[y] I think it deserves to be noticed that the weights of an atom of each of the three magnetic metals very nearly coincide. They are as follows:—

Iron	7·143
Nickel	7·305
Cobalt	7·326

They agree likewise in the number and constitution of their oxides, which are as follows :—

	Iron.	Nickel.	Cobalt.
First oxide	$1 i + 2 o$	$1 n + 2 o$	$1 c + 2 o$
Second oxide ...	$1 i + 3 o$	$1 n + 3 o$	$1 c + 3 o$

I have no doubt that the composition of their carburets, phosphurets, and sulphurets, will likewise be found to agree.

[z] There was an arithmetical error in the original number in the first table.

[a] According to the experiments of Bucholz, modified a little by the subsequent experiments of Berzelius described in the *Annals of Philosophy*, vol. iii. p. 100.

[b] This results from the experiments of Berzelius, who found the two oxides of tungsten composed respectively of 100 metal + 16·5 oxygen and 100 metal + 24·75 oxygen (*Annals*, vol. iii. p. 244). The first oxide is brown, and burns spontaneously when exposed to the air. It was obtained by passing a current of hydrogen gas through red-hot tungstic acid. The second is tungstic acid. Berzelius has shown that the blue oxide of Bucholz is the same with tungstic acid. It is obvious that the numbers 16·5 and 24·75 are to each other as $2 : 3$.

[c] The data are the same as those I used before ; but there is a slight arithmetical error in the number given in my first table.

[d] Berzelius has shown that there are three oxides of chromium (*Annals of Philosophy*, vol. iii. p. 101), the green, the brown, and the acid. The first contains 100 metal + 42·37 oxygen, while the acid is composed of 100 metal + 84·74 oxygen. These numbers

		Weight of an atom.
288	Titanium	18·010 [e]
289	Rhodium	14·903 [f]

	Number of atoms.	Weight of an integrant particle.
Oxide of silver	1 s + 1 o	... 14·714
Deutoxide of iron	1 i + 2 o	.... 9·143
Peroxide of iron	1 i + 3 o	...10·143
Deutoxide of nickel	1 n + 2 o	.... 9·305
Peroxide of nickel	1 n + 3 o	...10·305
Oxide of zinc	1 z + 1 o	.... 5·095
1st oxide of antimony	1 a + 3 o	....14·249
White oxide of antimony	1 a + 4 o	....15·249
Antimonic acid	1 a + 6 o?	..17·249
Oxide of tellurium	1 t + 1 o	.... 5·027
Protoxide of manganese	1 m + 1 o	.... 8·115
Deutoxide of manganese	1 m + 2 o	.... 9·115
Tritoxide of manganese......	1 m + 3 o	...10·115
Peroxide of manganese	1 m + 4 o	...11·115
Deutoxide of molybdenum ...	1 m + 2 o	.... 8·013
Peroxide of molybdenum	1 m + 3 o	.... 9·013
Deutoxide of tungsten	1 t + 2 o	...14·121
Peroxide of tungsten........	1 t + 3 o	...15·121
Deutoxide of cerium	1 c + 2 o	...13·487
Peroxide of cerium	1 c + 3 o	...14·487
290 Green oxide of chromium ...	1 c + 2 o	.... 6·720
291 Brown oxide of chromium ...	1 c + 3 o	.... 7·720
292 Chromic acid	1 c + 4 o	.... 8·720
293 Protoxide of titanium	1 t + 1 o	...19·010

are to each other as one to two. As the brown oxide is interme-
diate, we must suppose the green oxide a compound of 1 atom
metal + 2 atoms oxygen, the brown of 1 atom metal + 3 atoms
oxygen, and the acid of 1 atom metal + 4 atoms oxygen. After
all, I think it most likely that the brown oxide is merely a com-
pound of the green oxide and the acid. In that case we must
reduce the weight of an atom of chromium to 2·360.

[e] According to Berzelius, from an experiment of Richter. See
Annals of Philosophy, vol. iii. p. 251.

[f] Berzelius has shown (*Annals of Philosophy*, vol. iii. p. 252)
that there are three oxides of rhodium. The first, existing in the
muriate, is composed of 100 metal + 6·71 oxygen; the second, of
a flea-brown colour, obtained by heat, of 100 metal + 13·42 ;
the third, red, obtained by precipitation from soda-muriate of
rhodium, and containing more oxygen than either of the other two.

	Number of atoms.	Weight of an integrant particle.
294 Peroxide of titanium	1 t + 2 o	20·010
295 Protoxide of rhodium	1 rh + 1 o	15·903
296 Deutoxide of rhodium	1 rh + 2 o	16·903
297 Peroxide of rhodium	1 rh + 3 o	17·903

These are all the alterations and additions which I can venture at present to make upon my original table, published in the first number of the second volume of the *Annals of Philosophy.* I have prefixed numbers to all the additional substances introduced in the present table, partly for the sake of easy reference, and partly that the reader may be able at a glance to distinguish those bodies the weights of whose atoms have been altered. I am far from conceiving that, even with these alterations, I have attained to any thing like precision ; but it is only by constant corrections that we can expect, in such a science as chemistry, to approximate at last sufficiently near the truth for all practical purposes.

Article III.

On the Composition of Blende. By Thomas Thomson, M.D. F.R.S.

As far as is known at present, zinc, though a very abundant metal, enters into a much smaller number of mineral combinations than lead or copper or even silver. It has been found only in four states constituting the four following species :

1. Blende.
2. Hydrous carbonate of zinc.
3. Anhydrous carbonate.
4. Silicated zinc.

In the last three of these species, which are usually confounded together under the name of *calamine,* the zinc is in the state of an oxide ; but in blende it occurs in the metallic state.

Mr. Smithson, some years ago, determined the composition of the last three species of the ores of zinc with sufficient accuracy. But if we except the experiments of Bergman, made in 1779, when chemical analysis had not acquired the requisite exactness to decide upon the composition of complicated minerals, and a single analysis by Dr. Kidd, published two or three years ago, we are not in possession of any well established facts respecting the composition of blende. On that account, I conceive it will be worth while to lay before the public the result of some experiments which I made lately, in order to satisfy myself upon this point.

Blende is an ore of zinc, which almost always accompanies galena or sulphuret of lead, and which by the English miners is

distinguished by the name of *black jack*. It is much more common in veins than in any other situation ; and as galena abounds most in transition rocks, the same rule holds with blende. Thus it is found at Lead Hills, which are situated in a transition country, and accompanies the lead ore which is so abundant in the north of England, and which occurs likewise in a transition country. But we must not suppose that galena and blende are confined to transition districts ; for we find lead mines in Great Britain both in primitive and floetz formations. Thus the lead mine of Strontian in Argyleshire is a vein traversing a gneiss mountain. At Huel Anne in Cornwall both galena and blende occur in a vein which traverses clay slate. But it is daily becoming more and more probable, that all the granite and clay slate in Cornwall belong to the class of transition rocks. On the other hand, Derbyshire, so productive in lead mines, and containing many veins which yield both galena and blende, seems to belong altogether to the class of floetz rocks.

I have never myself seen any vein completely filled with blende. It was always accompanied by other minerals ; and galena and fluor spar are almost never failing attendants.

We cannot recognize either in Theophrastus or Pliny, any description which would lead us to suppose that blende had been distinguished by the ancients as a peculiar mineral. The first notice of it which I have met with occurs in the treatise of Agricola, *De Natura Fossilium*, published, I believe, in 1546. He gives it the name of *galena inanis*, and mentions it in the following terms. *Galena vero inanis, sive lapis plumbarius inanis, lucet in nigrore, splendetque ipsa plumbario lapide, et arenæ colore similis, omnino omnis argenti et plumbo expers. Quorum partem, si in fornace fuerit simul conjecta, disperdit ac dissipat.* De Natura Fossilium, lib. x. p. 365. The term *galena inanis* may be considered as a kind of Latin translation of the German word *blende*, which was given to this mineral by the miners, because it had a considerable resemblance to galena, but no lead can be extracted from it. For the same reason, it was distinguished among mineralogical writers by the name of *pseudo-galena*.

Mineralogists, after they began to arrange minerals into an artificial system, were long at a loss about the position of blende ; because they were ignorant of its composition. Brandt, a celebrated Swedish chemist, showed in 1735, that it contained zinc ; and soon after, Von Swab pointed out a method of extracting that metal from blende.[*] I cannot find any notice of blende in the long dissertation on zinc by Mr. Pott. Indeed he expressly says, that there are no ores of zinc. Of course he must have been ignorant both of the composition of calamine and blende. But in 1746, Margraaf informs us that blende contains zinc, and describes an easy process by which that metal may be obtained from it.[†] Mar-

* Bergman, Opusc. ii. 313.   † Margraaf, Opusc. i. 101.

graaf does not expressly mention the presence of sulphur in this mineral; though from his treatment of it, and his description of the process, it follows clearly, I conceive, that he was acquainted with its existence. Cartheuser, in his Mineralogy, published in 1755, says, that besides zinc and sulphur it contains iron and arsenic.* In 1779, Bergman published a chemical analysis of the ores of zinc. He examined many varieties of blende, and found them composed of zinc, iron, sulphur, arsenic, alumina, and silica. †

The French chemists, after the introduction of the Lavoisierian theory, relying upon the difficulty of uniting zinc and sulphur together by heat, considered blende as a sulphureted oxide of zinc. This opinion was first called in question by Proust, who having analysed some specimens of that ore, found that the zinc must exist in it in the metallic state. ‡ An analysis of brown blende, which I made soon after, induced me to adopt the same conclusion. § The analysis of Dr. Kidd leads directly to the same opinion, and indeed is inconsistent with any other. I believe it is generally received at present both by chemists and mineralogists: hence if blende be a sulphuret of zinc, an accurate analysis of it would make us acquainted with the proportion in which sulphur and this metal combine, which has not yet been determined by satisfactory experiments. There can be no doubt, from the table of sulphurets given in the second volume of the *Annals of Philosophy*, that an atom of all the metals either combines with one atom or with two atoms of sulphur. But though blende was found in that table to approach to a compound of one atom zinc and one atom sulphur, it did not exactly agree with the weights of these atoms as determined by other data. This want of coincidence might be owing to errors in the analysis of blende, or to mistakes into which I had fallen in determining the weight of an atom of zinc. In order to ascertain this point, and likewise to see whether the iron which blende contains exists in the state of an oxide or metal, and whether it be combined with sulphur, I made several very careful analyses of that ore, and was not satisfied till I obtained the exact weight of the blende originally employed, which is much more difficult than those not conversant in chemical analyses could imagine.

Werner describes three subspecies of blende, distinguished from each other chiefly by their colour, namely, the *yellow, brown,* and *black.* I have not yet subjected black blende to analysis; but I find that the yellow and the brown subspecies agree nearly in their composition. The only difference is that the brown contains more iron than the yellow. I have never analysed any yellow blende perfectly free from iron. The purest specimen I have met with contained $1\frac{1}{4}$ per cent. of that metal, while specimens of brown

* Cartheuser's Mineralogy, p. 61.　　　† Opusc. ii. 389.
‡ Jour. de Phys. lvi. 79.　　　§ Jameson's Mineralogy, ii.

blende contained more than 12 per cent. Hence I think there can be no doubt that the yellow blende is the purest. Probably some of the transparent and crystallized specimens of it may be altogether free from iron; though none such have yet fallen into my hands.

As my method of analysis was always the same, I consider it as unnecessary to detail any more than one experiment; and I shall make choice of the one with the exactness of which I was myself most satisfied. The specimen was from Huel Anne in Cornwall, and was picked up by myself on the spot. It was not crystallized; but I made choice of it as in appearance quite free from all admixture of earthy or foreign matter of any kind.

Its colour was blackish brown. The lustre splendent and semimetallic. The fracture foliated; but the six fold cleavage, which usually characterizes blende, could not be recognized in it, owing to its being composed of granular concretions. It was opaque. The streak was yellowish brown. It possessed the usual degree of hardness of blende. Its specific gravity was 4·049.

1. Fifty grains of this zinc were reduced to a fine powder,* and digested in a flask for two days with diluted nitric acid. At first the action was pretty violent, and nitrous gas was discharged copiously; but after some hours it ceased entirely. The ore was then floating upon the surface of the liquid, and appeared coated over with sulphur. The whole was thrown upon a filter, and the undissolved residuum being well washed and dried in the open air, was exposed for 24 hours upon an iron of the temperature of rather more than 110°. Thus dried, it weighed 26 grains.

2. These 26 grains were put upon a watch glass and exposed to the heat of a lamp. The matter underwent an incipient fusion, emitted a white smoke, and at last took fire and burnt with a blue flame, giving out a strong odour of sulphur. When all appearance of combustion and smoke was at an end, the glass was allowed to cool. The matter, being now weighed, was found to amount to 22·4 grains. The 3·6 grains of loss were considered as sulphur dissipated by the heat.

3. The 22·4 grains of residue, which had now assumed the appearance of the original blende, were put into a flask and digested two days with diluted nitric acid. The second day, as all action was over, and the whole very nearly dissolved, the flask was exposed for some hours to a heat of about 130°. The whole was then thrown upon a filter. The undissolved portion was well washed and dried. It weighed 0.54 grain.

4. This small residue was put upon a watch glass and exposed to the heat of a lamp. It emitted a visible smoke having the odour of sulphur. When cold its weight was reduced to 0·38 grain. The 0·16 grain of loss was considered as sulphur dissipated by the heat.

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* I found in the preceding analyses, that unless the blende be reduced to powder, the experiments are liable to considerable loss.

5. The remaining 0·38 grain had the appearance of small grains of quartz, mixed with a particle or two of undecomposed blende. To remove this portion of ore the whole was digested in diluted nitric acid for a day. The residue was now pure quartz sand, and weighed 0·3 grain. The liquid containing the 0·08 grain of blende in solution was accidentally lost. On that account, I shall consider the quartz sand as amounting to 0·38 grain.

6. The two portions of acid liquid which had been digested on the ore, and which contained the greatest part of it in solution, were mixed together, and almost, but not quite, saturated with carbonate of soda. A considerable excess of caustic ammonia was then poured in. By this alkali the oxide of iron was thrown down in yellowish red flocks, while the whole of the zinc was held in solution. The oxide of iron being separated by the filter, washed, dried, and heated to redness, weighed 8·5 grains. Now this is equivalent to 5·98 grains of metallic iron.

7. The residual liquid was now boiled in a glass retort down to half its bulk, in order to drive off the ammonia and precipitate the oxide of zinc. About one half of that oxide precipitates after a few minutes boiling; but it requires considerable concentration before the other half falls down. From this circumstance it is not improbable, that ammonia and oxide of zinc unite in two proportions. The oxide of zinc thus obtained, being washed, dried, and heated to redness, weighed 36·4 grains. This is equivalent to 29·32 grains of metallic zinc. It may be proper to mention, that the oxide of zinc thus obtained was not quite white, but had a slight tinge of green. I conceived that this might be owing to the presence of copper; but if this metal was actually present, it was in too minute a quantity to be detected by the usual tests.

8. The liquid thus freed from iron and zinc was mixed with nitric acid till it acquired a perceptibly sour taste. This was done to prevent any inaccuracy from the presence of ammonia, if any should still remain in the liquid. A solution of muriate of barytes was then mixed with it. The sulphate of barytes which precipitated, being washed, dried, and heated to redness, weighed 77·616 grains. Now 77·616 grains of sulphate of barytes contain 26·4 grains of sulphuric acid, or 10·56 grains of sulphur.

From the preceding analysis it appears that blende is composed of the following ingredients:

Zinc	29·32	58·64
Sulphur	14·32	28·64
Iron	5·98	11·96
Quartz	0·38	0·76
	50·00	100·00

But as the quartz was only mixed with the ore in grains, it may be abstracted. In that case brown blende is composed of

Zinc .... 59·09
Sulphur .... 28·86
Iron ... . 12·05
———
100·00

From the result of this analysis, which was fully confirmed by several others, I consider myself as warranted to conclude, that both the zinc and the iron exist in blende in the metallic state. I think it equally evident, that the iron is not combined with sulphur, but only the zinc; for if we suppose that one atom of zinc combined with one atom of sulphur constitutes sulphuret of zinc, and if we assume the weight of an atom of zinc at 4·139, and an atom of sulphur at 2, according to my previous determination of these weights, in that case 59·09 of zinc ought to combine with 28·55 of sulphur. Now this very nearly agrees with the numbers in the preceding table. Sufficiently so at least to demonstrate that none of the sulphur can be in combination with the iron.

If we consider the preceding experiments as correct, and they were made with so much care that I am myself disposed to rely on them, sulphuret of zinc is composed of

Zinc .... 67·19 ...... 100· ...... 214·40
Sulphur .. 32 81 ...... 48·84 ...... 100 00
——— ——— ———
100·00 148·84 314·40

Were we to suppose this determination rigidly accurate, it would occasion a small alteration in the weight of an atom of zinc, and likewise in the constitution of oxide of zinc. I am disposed to put more confidence in it than in my preceding experiments to determine the composition of oxide of zinc by dissolving that metal in nitric acid, and weighing the oxide obtained, or by dissolving it in sulphuric acid, and calculating from the proportion of zinc dissolved how much oxygen it must have combined with; for the zinc of commerce, upon which my experiments were made, is never quite free from lead. And though I endeavoured to determine the quantity of that metal present, and to allow for it, such a determination was liable to uncertainties, from which the analysis of blende was free.

I shall therefore henceforth consider an atom of zinc as weighing 4·095, and oxide of zinc as composed of 100 metal + 24·42 oxygen. What renders me more disposed to adopt these new numbers is, that they approach nearer to the determination of Berzelius than any of my preceding numbers. And the accuracy of his experiments I have had occasion in many repetitions of them, to admire as most extraordinary. He found oxide of zinc composed of 100 metal + 24·4 oxygen. My first determination was 100 metal + 23·5 oxygen; my second 100 metal + 24·16 oxygen;

and the present 100 metal + 24·42 oxygen; each approaching nearer than the preceding to the numbers of Berzelius.

ARTICLE IV.

On the Composition of Sulphuret of Antimony.   By Thomas Thomson, M.D. F.R.S.

SULPHURET of antimony is a mineral that has been so long and so universally known to all my readers, that I consider it as quite superfluous to give any description of it.   It is mentioned both by Dioscorides and Pliny as a metallic substance ; though neither of them had any idea of its composition.   Agricola, in his treatise De Natura Fossilium, gives a particular description of it, and shows how it may be distinguished from other minerals to which it has some resemblance.   He knew that the metal called *antimony* could be extracted from it, for a process had been published long before by Basil Valentine ; but there is nothing in his writings to lead us to suppose that he was acquainted with the real nature of this mineral.

The same remark, as far as my observation goes, applies to all the chemical and mineralogical writers who followed Agricola, during a period of nearly 200 years.   The first person who ascertained that *crude antimony* is a simple compound of metallic antimony and sulphur, I conceive was Meuder, who published at Dresden, in 1738, a dissertation entitled Analysis Antimonii Physico-chym. Rationalis, in which he establishes the true composition of the mineral in question by satisfactory experiments.   Soon after we find this composition recognised by Pott and Margraaf as an established and well known fact.

The first person who endeavoured to ascertain the proportion of the constituents of this mineral was Bergman, in his dissertation entitled De Antimonialibus Sulphuratis, first published in 1782. His method was to dissolve sulphuret of antimony in a mixture of three parts muriatic and one part nitric acid.   Such a mixture, according to him, dissolves the antimony without touching the sulphur.   Towards the end of the process it is necessary to apply heat, in order to free the sulphur completely from antimony. I conceive it to be possible by this method to dissolve the whole antimony without touching the sulphur ; but the chances against complete success are almost infinite, unless we be previously acquainted with the proportion of metallic antimony present.   That is, unless we are previously acquainted with the very point to be determined.   For if we use too much nitric acid, part of the sulphur will be converted into sulphuric acid ; and if we use too little, the muriatic acid will come into action as a solvent, and a portion

of the sulphur will make its escape in the state of sulphureted hydrogen gas. The result of Bergman's analysis was that native sulphuret of antimony is composed of

$$
\begin{array}{llll}
\text{Antimony} & \dots\dots\dots\dots & 74 & \dots\dots\dots 100\cdot000 \\
\text{Sulphur} & \dots\dots\dots\dots & 26 & \dots\dots\dots 35\cdot035 \\
\hline
& & 100* \\
\end{array}
$$

Wenzel indeed, in his Verwandschaft, first published in 1776, had endeavoured to determine the proportion in which antimony and sulphur combine, by heating these two bodies together in a covered crucible, and ascertaining the increase of weight which the antimony sustained. The result of this experiment was, that his artificial sulphuret of antimony is composed of

$$
\begin{array}{llll}
\text{Antimony} & \dots\dots\dots\dots & 77 & \dots\dots\dots 100 \\
\text{Sulphur} & \dots\dots\dots\dots & 23 & \dots\dots\dots 29\ 87 \\
\hline
& & 100 \\
\end{array}
$$

But this method is scarcely ever to be depended on, unless it be attended with precautions, which Wenzel at the time when he made his experiments could not consider as necessary.

Karsten, in his Mineralogical Tables, published in Berlin in 1808, quotes no other analysis of this ore, except the one by Bergman which I have specified above : hence it would appear, that at that period he was not aware of the existence of any other. No analysis whatever is given by Haüy in his System of Mineralogy.

In the Journal de Physique, lv. 325, Proust gives the result of his experiments on sulphuret of antimony, and deduces from them that it is a compound of

$$
\begin{array}{llll}
\text{Antimony} & \dots\dots\dots\dots & 75 & \dots\dots\dots 100 \\
\text{Sulphur} & \dots\dots\dots\dots & 25 & \dots\dots\dots 33\cdot333 \\
\hline
& & 100 \\
\end{array}
$$

Mr. John Davy, in his paper On the Combination of different Metals with Chlorine, published in the Philosophical Transactions for 1812, gives us sulphuret of antimony as a compound of

$$
\begin{array}{lllll}
\text{Antimony} & \dots 42\cdot5 & \dots\dots 74\cdot06 & \dots\dots 100 \\
\text{Sulphur} & \dots 14\ 86 & \dots\dots 25\cdot94 & \dots\dots 34\cdot96 \\
\hline
& & & 100\cdot00 \\
\end{array}
$$

Finally, Berzelius, in the second volume of his Lärbok i Kemien, p. 162, published at Stockholm in 1812, states, without giving any authority, from which one is disposed to consider it as the result of his own experiments, that sulphuret of antimony is composed of

* Opusc. iii. 167.

Antimony .... 72·91 100
Sulphur .... 27·09 37
                    ‾‾‾‾‾
                    100·00

To finish this historical sketch, it only remains to be stated, that Vauquelin some years ago made a set of experiments to determine with how much sulphur the different metals could be made to combine, when fused with it in a covered vessel. Among his other results, he found that artificial sulphuret of Antimony is composed of

Antimony 75 100
Sulphur .... 25 .... 33·333
                       ‾‾‾‾
                       100*

This result coincides exactly with the previous determination of Proust.

Such, as far as I am acquainted with the subject, are the experiments hitherto made to determine the composition of sulphuret of antimony. They do not differ from each other so much as might have been expected, considering the imperfection of the data, according to which some of these experiments were conducted. The following table exhibits a synoptical view of these experiments, beginning with Wenzel who gives the smallest proportion of sulphur, and terminating with Berzelius, who gives the greatest proportion of that constituent.

	Antimony.	Sulphur.
Wenzel	100 +	29·870
Proust	100 +	33·333
Vauquelin	100 +	33·333
John Davy	100 +	34·960
Bergman	100 +	35·035
Berzelius	100 +	37·000

The results of Wenzel and Berzelius differ considerably from each other, and likewise from all the rest. But if we were to leave them out, the other four experimenters approach pretty nearly to each other. If we omit Wenzel's experiment, which deviates most from the others, and bears the greatest appearance of inaccuracy, the mean of all the others gives us 35·572, for the proportion of sulphur combined with 100 antimony in sulphuret of antimony. Now this number in all probability is not far from the truth.

I thought it worth while to institute a set of new experiments, to investigate the composition of this native sulphuret with as much precision as possible; because an accurate knowledge of it would be of considerable assistance to us, in enabling us to determine the

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* Ann. de Mus. d'Hist. Nat. xvii. 135.

weight of an atom of antimony; a problem of some difficulty, on account of the impossibility of hitherto obtaining the oxides of that metal in a pure and uncombined state, so as to determine their composition with the requisite precision.

I employed a pure specimen of the common radiated variety, which I had in my cabinet, and which I believe was found in the south of Scotland; though I am not quite certain of this. The object which I had in view, was to ascertain the whole of the sulphur which the specimen contained. It is obvious, that as no foreign matter was present, the knowledge of this would be sufficient to determine the composition of the sulphuret.

It is well known, that when muriatic acid is boiled upon this sulphuret in powder, sulphureted hydrogen gas is given out in abundance, while the oxide of antimony is taken up by the acid. It would appear therefore, that in this case, the ore and the acid determine the decomposition of a quantity of water, the hydrogen of which combines with the sulphur, and flies off in the state of gas, while the oxygen unites with the metal and converts it into an oxide, which is taken up by the acid. It occurred to me, in consequence of my knowledge of this, that in order to know correctly the composition of sulphuret of antimony, it would be merely necessary to decompose a determinate weight of it by means of muriatic acid and heat, and receive the sulphureted hydrogen gas over mercury. The knowledge of the quantity of this gas obtained, would not only show the quantity of sulphur in the ore, but enable me also to determine the quantity of oxygen which had united with the metal. But upon repeating this experiment two or three times, I found that the analysis of the ore in this way could not be depended on. For there always sublimed into the neck of the retort a quantity of *golden sulphur of antimony* (probably a hydrosulphureted antimonial oxide) which I could neither collect with precision, nor allow for in a satisfactory manner.

Disappointed in this process, I had recourse to the well known action of nitro-muriatic acid upon sulphuret of antimony. Into a large phial I put 100 grains of the pure ore in the state of a fine powder, and poured over it a quantity of nitro-muriatic acid. When the action was over, I decanted off the liquid portion, and treated the undissolved residue with fresh portions of acid, till I had reason to be satisfied that all the metallic part of the ore was taken up. The residuum being now dried had all the appearance of sulphur. It weighed ten grains, and when kindled, burnt all away with a blue flame, leaving no appretiable residue.

The acid solutions being mixed together, were poured into about a quart of distilled water, in order to precipitate the oxide of antimony. For greater security, I then saturated the acid in this watery solution with ammonia. The white precipitate, being separated by the filter, was well washed and dried. In this state it weighed 93·5 grains; and as far as I could judge, was a pure oxide of antimony.

The watery liquid thus freed from the antimony, still contained a considerable portion of sulphur, which had been acidified by the action of the nitric acid. Accordingly, when muriate of barytes was dropt in, a copious precipitate of sulphate of barytes fell. This precipitate, when washed and dried, weighed 119·3 grains; equivalent to 40·57 grains of sulphuric acid, or to 16·23 grains of sulphur.

From the preceding analysis it appears that 100 parts of sulphuret of antimony contain 26·23 parts of sulphur: therefore, the sulphuret in question is composed of

$$\begin{array}{lll}
\text{Antimony} \dots\dots\dots 73\cdot77 \dots\dots 100\cdot000 \\
\text{Sulphur} \dots\dots\dots\dots 26\cdot23 \dots\dots 35\cdot556
\end{array}$$

This result approaches most nearly to the analysis of Bergman. The reader will observe too, that it almost coincides with the mean of all the preceding analyses, if we leave out that of Wenzel. It was this circumstance that gave me confidence in it, and induced me to consider it as approaching more nearly to precision, than any of the analyses offered to the public by preceding chemists.

Let us suppose then that sulphuret of antimony is composed as I have just stated; and let us deduce from that composition the weight of an atom of antimony. If we suppose, as I have done (*Annals of Philosophy*, ii. 112) that this sulphuret is a compound of two atoms sulphur and one atom antimony, we have this analogy, 35·556 : 100 :: 4 : *an atom of antimony*. This would give us the weight of an atom of antimony 11·249. This is a very little more than I originally made it in my first table, deduced from the analysis of the oxides of antimony; analyses attended with so much difficulty, that implicit confidence could not be put in them.

The 93·5 grains of white oxide of antimony, obtained in the preceding analysis, obviously contained 19·73 grains of oxygen; so that this oxide of antimony is composed of 100 metal + 26·745 oxygen. This is the oxide which Berzelius has distinguished by the name of *antimonious acid*. For when heated to redness in a platinum crucible, it becomes of a dazzling whiteness, and exhibits the other properties of that substance which he has described. My analysis of it agrees nearly with the previous analysis of it by Mr. John Davy, who found it a compound of 100 metal + 26·471 oxygen. Now if we suppose it a compound of one atom metal and three atoms oxygen, which was the original supposition that I set out with in my first table, we shall find that this very nearly coincides with the above analysis: for 11·249 : 3 :: 100 : 26·669. Now this differs very little from the result of my analysis. If we were to consider the analysis of the oxides of antimony by Berzelius, in his paper on the Cause of Chemical Proportions, published in the preceding volume of the *Annals of Philosophy*, as correct, the weight of an atom of antimony would be 16·129. But I must confess, that several of his conclusions seem to me founded upon too slender analogies to be entitled to confidence. I

think therefore it will be safer, in the present state of our knowledge, to deduce the weight of an atom of antimony from the sulphuret of that metal, with the composition of which we are accurately acquainted, than from imperfect deductions respecting oxides which have not hitherto been subjected to a satisfactory examination.

Article V.

Chemical Analysis of Caviar. By Professor Dr. John, of Frankfort-on-the-Oder.*

Caviar, as is known, is formed from the roe of some species of fishes, especially some species of *sturgeons,* as the Accipenser sturio, A. huso, A. rathenus.   It comes in a two-fold state, from Russia where it was probably prepared,† to us in Germany.   Two kinds of caviar are distinguished in commerce, namely, the pressed and fresh caviar.   Now this last kind, as it consists of the roes of the fish unaltered (some sturgeons yield 200 pounds of this substance), when it is subjected to analysis, will yield a result more to be depended on than the pressed caviar, which is not only mixed with foreign matter, as common salt, but which, by being dried and subjected to pressure, has undergone a real change in its nature. There is a good description of the method of preparing the pressed caviar in Krünitz' Encyclopedia.   I employed in the following analysis true Russian caviar, the roes of which had been pressed together in consequence of the distant carriage, but which had undergone no fermentation, nor any other alteration.

I rubbed 420 grains of this fresh caviar with water into a thin pap, and then mixed it with a greater proportion of that liquid.   I obtained a solution similar to an emulsion, from which the membranous part of the caviar gradually precipitated to the bottom of the vessel. It was obvious from the appearance of the solution that it contained an oily matter mixed with it.   The whole was poured upon the filter, through which there passed a pretty transparent and colourless solution.   The residuum which remained upon the filter was treated with water in the same manner as at first, by which the dissolving power of the water seemed to be exhausted.   The solution in water, being evaporated, coagulated, and deposited a white substance similar to the curdy part of milk.   Alcohol, being dropped into the liquid, contributed to the coagulation and precipitation of this substance.   This substance being separated by the filter, and dried in a hot place, assumed the form of a transparent, brittle,

† Several other fish furnish a substance similar to common caviar, and it is prepared in different parts of Prussia and Pomerania.

horny-looking substance, equally insoluble in water and alcohol. Pure caustic potash ley dissolved it, and it was thrown down again by the addition of an acid. This substance, which possesses the properties of albumen, when fully dried, weighed 26 grains.

The watery solution thus freed from albumen, being properly concentrated, deposited 26 grains of common salt in small cubes, mixed with a little alkaline sulphate. The mother ley contained a portion of slime scarce capable of being weighed. It was mixed with common salt, but easily separated by washing it with water.

I now allowed the portion of caviar which was insoluble in water to dry, by exposing it to a very gentle heat. In this state it still retained very strongly its peculiar taste. It was repeatedly digested in alcohol, the solution was mixed with water, heated, and allowed to cool again. By this means 18 grains of an unctuous yellow coloured oil were obtained. The residue left by the alcohol undissolved had lost the peculiar taste of caviar. The liquid freed from the oil was found to contain nothing but an atom of common salt.

What the alcohol had left undissolved was boiled for an hour in pure water. The filtered water, being evaporated to dryness, left only an atom of jelly. The portion undissolved in water weighed, after being dried, 104 grains. It had the colour of caviar, and was indurated albumen. When burnt it emitted a very fetid animal smoke. The coal which remained behind was burnt to ashes. The ashes weighed 1½ grain. They contained no alkali; but consisted of common salt, sulphate of potash, phosphate of lime, and a trace of iron.

The constituents furnished by the preceding analysis of 420 grains of caviar are the following :—

	Grains.
Pure dry albumen from the solution of caviar	26
Unctuous oil	18
Insoluble or indurated albumen	102
Common salt with some alkaline sulphate	28
Jelly	⎫
Phosphate of lime	⎬ 2
Oxide of iron	⎭
Water as the deficiency	244
	——
	420

From the preceding analysis it follows that caviar contains a notable portion of soluble albumen, which constitutes so nourishing an article of food, that the peculiar taste of caviar is to be ascribed to an oil, which is the same as that contained in all the ova of animals so far as I have hitherto examined them, namely, in those of insects (butterflies and grasshoppers), of amphibia, and of birds

in considerable quantity; and, finally, that the albumen in caviar exists in two states, namely, soluble and insoluble in water, but that the insoluble portion is by far predominant, and in a much greater quantity than in any other ova hitherto examined by me.

Article VI.

Observations on the Heat of Springs, and on Vegetation, in order to determine the Temperature of the Earth and the Climate of Sweden. By George Wahlenberg, M.D. Fellow of the Royal Academy of Sciences of Stockholm.

III.

Observations on the Heat of Springs, and on the Nature and Distribution of Plants in the Northern Provinces of Sweden, in order to determine the Climate of these Parts. By George Wahlenberg.*

. As the variable springs reach the lowest point of their temperature in spring, and the highest point towards the end of August, or somewhat later, it is only requisite for us to make some observations on them during the few summer months in order to discover the whole scale of their variations. The Royal Academy of Sciences at Stockholm recommended these observations to my attention during my journey to the mountains of *Umeo Lappmark*, and requested me, both on my way thither, and as I returned again, to examine the temperature of all the springs that I happened to meet with. I left Upsala in the middle of June, 1810, and returned to it again at the end of August.

I arrange the observations which I made during that journey according to the provinces over which I travelled.

Mundung, on the river Dal; N. lat. 60½°.

There is a fine spring at *Mehede*, which rises at the bottom of a rock. Its temperature was, on the 18th June, 42·08°; on the 24th August, 42·26°. It is therefore as constant in its temperature as can be expected in a spring.

A spring at *Grimsarbo* gave, on the 18th June, 41·90; but on the 24th August, 46·04°.

The temperature of 42·26° is very low, when compared with that of Upsala, which is 43·70°. It is but seldom that we have an opportunity during a single journey to observe so great a difference

in the temperature of the earth at the same height above the level of the sea. The nature of the vegetation, however, appears fully to correspond with this difference. This can be observed with more ease, as scarcely in any other place can the regular diminution of vegetation be found so exact as along the north coast of the Gulph of Bothnia. Thus the difference between the flora of Upland and Lapland is very striking. Of the 642 perfect plants around Upsala, 318 are wanting in Lapland; while, on the other hand, of the 500 Lapland plants, 134 are wanting at Upsala. Hence in a journey to the north every second plant appears for the last time, and every fifth plant is new. As these changes in fact take place only gradually, it is obvious that there must be a great number both of northern and southern limits of plants, which will put it in our power to judge of the climate. We cannot, however, assign the limit where a plant appears for the last time; because that often depends upon accidental circumstances, but only the place where it ceases to be general. On that account scarce plants are not proper for such determinations.

Of the plants which grow round Upsala, the following are wanting in *Dal-elf*:—*Anemone pulsatilla, delphinium consolidum, melampysum cristatum, artemisia campestris, leonurus cardiaca, cynoglossum officinale, phalaris phleoides, prunus spinosa, mespilus cotoneaster.*

On the other hand, the following plants grow here, which do not make their appearance farther north. The *oak,* accidentally as far as *Tabodurna.* The great oaks in the islands of the river *Tostebo,* near *Starbron,* in the parish of *Hille,* are indebted for their flourishing state to local advantages. The *centaurea scabiosa* appears very seldom farther north. Here also appear for the last time *rhamnus catharcticus, cratægus oxyacantha, ornithogallum luteum.*

Other circumstances show the difference of climate between Upsala and the mouth of the river *Dal.* When in spring the fields round Upsala are become free from snow, we still find sledge roads at Tyerp. In the latter place the winter seed was already above ground in the end of August, at a time when they were scarcely thinking of sowing it at Upsala.

Geffle; N. lat. 60·75°.

Good springs are uncommon here. The *Sörbykälla* indeed gives out a sufficient quantity of water, but the place where it issues out of the earth is not sufficiently distinct. It was, on the 20th June, 42·44°; on the 23d August, 46·58°.

I was shown likewise an excellent spring a quarter of a Swedish mile south-east from Geffle, at *Brynaefsgard,* the temperature of which, on the 23d of August, was 41·9°. This we may consider, upon pretty good grounds, as the temperature of the earth at *Geffle.*

Half a Swedish mile south from the town, the *alnus incana* grows in abundance ; but in the whole of *Gestrikeland* this tree occurs much more rarely than the *alnus glutinosa*. Hence its limit may, without impropriety, be placed here. The *rubus chamæmorus* and *scirpus cæspitosus* likewise have their limit here. The southern plants which do not vegetate farther north than the latitude of Geffle are the following :—*Spiræa filipendula, saxifraga granulata, agrimonia eupatoria, trifolium montanum, malva rotundifolia, geranium rotundifolium* and *cicutarium*.

In the parish of *Hille*, one eighth of a Swedish mile north-east from *Trödje*, the temperature of the spring called *Kallkäll-Bach* was, on the 20th June, 41·72° ; on the 22d August, 43 52°.

This rivulet is remarkable, because the *cornus suecica* grows in it upon small hillocks of earth. This plant appears considerably later in *Helsingeland* ; a proof that springs are capable of promoting the growth of such northern plants as are not quite suited to the climate. A strong mineral spring at *Hamrong* had the following temperatures:—On the 20th June, 40·1° ; on the 22d August, 42·08°.

These temperatures appear low.

Gestrikeland at its northern limit (in lat. 61°) is exactly similar in point of vegetation to the country round the lake *Yngen*, in *Vermeland* (in lat. 59·5°, and 549 English feet above the level of the sea). *Betula nana* and *carex globularis* are quite common; the first of which, in the neighbourhood of *Trodje*, is only to be seen growing sparingly in morasses. The following plants find their northern limit at this place :—*Primula veris, corylus avellana, viburnum opulus, lythrum salicaria*. The *alnus incana* still retains its superiority over the *alnus glutinosa*.

Rubus arcticus appears upon our first entrance into *Helsingeland,* and, with other northern plants, becomes very common about *Skog*, which may be considered as the most elevated part of the province. After much trouble, I found here the spring called *Storswedjekälla*, on the left hand side of the way to *Goldgrube,* and one fourth of a Swedish mile from Strötjärn. It was a strong spring, and its temperature was, on the 22d August, 40·46°.

This may very well pass for the temperature of the earth in so high a situation.

Huddikswall; *lat.* 61·75°.

At *Enanger* we again reach the sea, and at that place, notwithstanding its more northerly position, we find nearly the same climate as about *Skog*. The temperature of *Siwikskälla*, a spring one fourth of a Swedish mile north from *Enanger*, was as follows: On the 23d June, 39·92° ; on the 20th August, 42·08°.

At *Tuna*, a little above *Huddikswall*, the *cornus suecica* becomes quite general, and the *alnus glutinosa* not only appears seldomer than the *alnus incana*, but is now quite rare, even upon the sea

shore. Observations on the springs in the town gave no correct results. *Björners* spring, on the north side of the town, was, on the 23d June, 39·74°; on the 20th August, 42·8°.

Ulsaters mountain spring was, on the 23d June, 38·66°; on the 19th August, 43·16°.

I found at last a better spring on the height in the parish of Hög, at a place called *Tannasäbodar*, on the left side of the way behind the forge of *Strömbacka*. It gave, on the 20th August, 40·28°; not much less than the little hill of *Skog*.

The spring *Skallnaess*, not far from *Mahlsta*, and in the division of *Frönlandsby*, was, on the 19th August, 40·82°.

Scabiosa suecisa grows here in abundance ; chiefly, however, in the interior of the country, and seldom on the sea shore, or the borders of rivers.

Dallsmyne spring, half a Swedish mile beyond *Mahlsta*, gave, on the 24th June, 40·46°; on the 19th August, 41·9°.

In the northern part of *Helsingeland*, in the parish of *Gnarp*, a spring on the hill *Oeran*, south from *Gryttje*, gave, on the 24th June, 39·02°; on the 19th August, 42·08°.

Another, at *Areskougen*, gave, on the 25th June, 39·2°; on the 18th August, 41·72°.

Medelped; *lat.* 62·5°.

In this province there are several perpetual springs. That of the mineral water called *Solomskälla*, in the parish of *Sättna*, was, on the 27th June, 39·02°; on the 16th August, 39·92°.

Dr. Zederström found its temperature, on the 12th July, 1803, 39·2°. An observation was likewise made on a good spring at *Hasans Swedjan*, south-west from that place, in the same parish. It was, on the 17th August, 39·2°.

We may therefore consider this as the true mean temperature of the earth for this place.

One quarter of a Swedish mile north-west from *Sundswall*, at *Storä*, in a morass, a spring gave, on the 18th August, 39·56°.

Another, situated not far from the first, on the 26th June, 39·38°; and on the 18th August, 41°.

People here are of opinion that a subterraneous stream flows hither from the north. It is certain that in these hilly districts the water retains its low temperature, though the ground is perhaps too much exposed to the influence of the sun's rays. That such a situation must have a considerable influence on the temperature of springs is evident from the strong spring of *Sjobergskälla*, at *Sundswall*, which lies on a fine southern declivity, and on the 26th June was of the temperature 41·72° ; and on the 18th August, 42·98°. In such a situation the high temperature of the earth remains long unaltered. *Hugskälla*, at the church of *Timmero*, was, on the 27th June, 41·86°; and on the 16th August, 41·72°. These constitute exceptions which must be neglected in a general view, as

well as the hazel bushes and the *primula veris*, to be seen at *Wattjon*, in *Tuna*.

Aconitum lycoctonum, a plant not quite unknown in *Helsinge-land*, now appears in abundance. On the other hand, *alnus gluti-nosa* has entirely disappeared. The following plants have their northern limit at this place:—*Galium verum*, *scabiosa arvensis*, *lonicera xylosteum*, *plantago media*, *geranium sanguineum*, *cam-panula perficifolia*, *thymus serpyllum*, *festuca fluitans*. On the north side of *Sundswall* apple-trees no longer bear fruit.

The small mountain range called *Skulskogen*, in the middle of *Angermannland* (lat. 63¼°), contains various southern plants which are not to be found on the north side of it. *Anemone hepatica*, *rosa canina*, *orobus tuberosus*, *heraclium sphondilium*, *centaurea jacea*, *dactylis glomerata*, *avena pratensis*.

I sought out the remarkable spring of *Skarbergskälla*, lying on the south side of *Utansjogardskäs*, or *Skuruberg*. It rises at the foot of the hill from under the rocks, which gave a name to the hill, and is completely screened from the action of the air and the sun. It springs out of the ground into a bason an ell deep, and gave, on the 28th June, 37·04°; on the 15th August, 38·12°: a temperature which, in all probability, is below the mean tempera-ture of the earth in this place.

Skulbergskälla, north-east from *Skulberg*, was, on the 14th August, 40·46°.

In *Angermannland*, on the north side of the wood, I found only one useful spring; namely, the one at the farm-house called *Onska*. The thermometer, being plunged into it, gave, on the 1st July, 38·66°; on the 12th August, 41·54°.

The granite of which the *Skulaberg* is composed is not favourable to the issuing out of the spring. The water does not sink deep into the ground, and therefore cannot penetrate and collect in the interior.

Vegetation shows that this place has a severer climate than the province on the south side of the wood. The following plants reach their northern limit in Själevand. *Anemone nemorosa*, *hypericum perforatum*, *pteris aquelina*. The want of these plants occasions a much greater uniformity in the appearance of the woods and meadows. *Salix arenarea* becomes now quite common, and this grey bush, seen on every side of the road, is no ornament to the country.

Veronica chamædrys and *carex leporina* appear for the last time at *Grundsunda*. Barley and rye still ripen at the usual time; but farther north rye ripens much later, though it has been sown the preceding year in the beginning of August. This circumstance gives us no favourable idea of the fertility of the earth in these high northern latitudes.

Umeo; *lat.* 64°.

The borders of the fields are still adorned with many beautiful

plants, which usually accompany agriculture ; as *potentilla anse-
rina* and *argentea, dianthus deltoides, pimpinella saxifraga, cen-
taurea cyanus, lycopsis arvensis.* The meadows, on the other hand,
have completely the aspect of Lapland, in consequence of the great
quantity of *cornus suecica* growing in them, and of the black
phleum alpinum bristling up every where. In low moist places we
see for the last time the *salix fusca* ; and the *myrica gale* is very
rare farther north. Of Lapland plants appear the *salix arbuscula*
and *splachnum luteum.* I first found good springs at *Tafuelsjö*, two
Swedish miles north-north-west from *Umeo.* The land rises very
gently, and may be here 100 feet above the level of the sea.
Woody hills of about 200 feet in height run along the sea shore.
At *Juckswallsbach*, or rather half a Swedish mile east from it, a
great many springs rise out of the ground not far from the stable
belonging to the house. Below these is the *Pottängskallä*, a very
strong mineral spring, rising in a level spot. I found its tempera-
ture, on the 5th July, 37·22° ; on the 8th August, 37·22°.

Another spring, at *Innerstrojningen*, higher up, and beyond the
rivulet, was, on the 5th July, 36·32 ; on the 8th August, 37·58°.

Lower down, but likewise on the north side of the rivulet, is the
spring of *Starkabo.* It was, on the 5th July, 38·66° ; on the 8th
August, 39·02°.

The first of these springs appears to give us the mean tempera-
ture of the earth at this place. The medium of the temperatures
of the second spring does not differ much, from its being 36·86° ;
but this deviates far from the temperature of the air which, from
five years' observations of Mr. Naezen, at Umeo, is only 33·38°.
The same difference exists at Uleo ; for the mean temperature of
the air at this place, according to Julin's observations as corrected
by Von Buch, is 33·21° ; but the temperature of a spring at Uleo,
on the 29th August, was 37·4°. This is probably the consequence
of the longer and more severe winter in these northern places.

As we retire from the shore of the gulf of Bothnia towards the
mountains of *Umeo Lappmark* the temperature and vegetation
diminish rapidly, because the land rises suddenly. On that
account *Tufwelsjö* constitutes the northern limit of many southern
plants. I saw *myrica gale* for the last time at *Hissjön.* *Calla
palustris* was seen only once at *Degerfors.* *Veronica officinalis,
festuca elatior, carex stellulata,* disappear here. *Rubus arcticus*
hardly reaches any farther north. At *Degerfors* and *Tegnaess,
chrysanthemum leucanthemum* and *fragaria vesca* were still to be
seen in small quantities ; but farther north they were not to be
found ; but their loss was not much felt, being replaced near the
houses by abundance of *matricaria inodora.* Here also *lysimachia
vulgaris* and *scutellaria galericulata* disappeared. *Phleum alpinum*
was become quite common. *Lychnis alpina* and *salix myrtilloides*
grew abundantly near *Tegsnaess.*

Between *Degerfors* and *Tegsnaess*, half a Swedish mile from the
latter, at a place called *Norderstängen,* a mineral spring rises,

which is perhaps the finest I have ever seen. It has opened to itself a way through the turf two feet deep, and coloured all around it of a deep red, from the pure ochre which it has deposited. Its temperature was, on the 5th August, 36·68°.

This is, without doubt, the true mean temperature of the earth in this place.

Lycksele; lat. 64·5°.

At last, after having gradually lost one half of the common plants of Upland, we reach the limits of Lapland. A few northern plants take the place of those that have disappeared. Most of the Lapland plants remain upon the heights of the mountains. On that account vegetation becomes poor and uniform. Those plants which are now most common in the meadows grow in Upland only in thick shady woods; as, for example, the *aira flexuosa* in dry places, and the *aira cæspitosa, arundo calamogrostis,* and *stricta,* in moist places. Some few meadow plants still make their appearance in the neighbourhood of houses, thus indicating beforehand that they are just about to disappear. These are *trifolium pratense, poa pratensis, ranunculus acris.*

The lake called *Fahl-Träsk* is the northern limit of *scirpus palustris, alisma plantago, lysimachia thyrsiflora, trifolium pratense.* *Northecium boreale* and *serratula alpina* grow abundantly in moist places.

How cold a soil so miserably covered with plants must be, it is easy to conceive. The colonists, who in the whole of *Norrland* consider the springs as all equal, and call them *cold springs,* begin now to lament bitterly over their appearance. They consider them as a plague to the land, and consider their appearance as a punishment for their sins, destined to destroy their husbandry. In fact, though these springs do not cool the air, it is evident that a field, through which water of the temperature 35·6° flows, is not calculated to produce rye, which requires for its growth a temperature of 46·4°. Springs are only beneficial to the fields when they possess this temperature. But the heat of springs at *Lycksele* does not exceed 35·6°. Below the great but short waterfall at the church of *Lycksele* lies the lake *Tansele.* On its south-western shore, a Swedish mile from *Lycksele,* is situated the colony *Tansele,* not far from which there are two springs. I found the temperature of eastern *Kallkällbäckens,* which rises in the middle of a wood, on the 9th July, 35·6°; on the 3d August, 35·96°.

West from it lies *Kyttmyrskälla.* It was, on the 9th July, 35·6°; on the 3d August, 35·78°.

Another spring, between the church of *Lycksele* and *Knafften-gard,* gave, on the 2d August, 36·14°.

These observations seem to establish 35·6° as the mean temperature of the earth at *Lycksele.*

I found the temperature of *Sjöbergs* spring, in Fahlträsk, on the 15th July, 37·4°; on the 27th July, 37·58°: and that of *Nyängs*

spring, at Atjärn, between *Fahlträsk* and *Bjerbäckan*, on the 16th and 27th July, 38·3°.

Gransele; lat. 65°.

The shores of the *Windelf*, at *Gransele* and *Gargelf*, on the *Gargelf*, are almost the highest places on which the inhabitants are engaged in earnest with agriculture. Even here, however, the rye generally ripens sufficiently to repay the trouble of the cultivator ; but they employ scarcely any means to promote the fruitfulness of the soil. It would be in vain to have recourse to the practice of fallowing ; for not a single blade of grass appears without culture, and in harvest the field is barer than before. The colonists are obliged to employ so great a quantity of manure that they complain of the want of it, being deprived of the assistance of their cattle during the greatest part of the year. Probably the cause of this is to be ascribed to the coldness of the earth. In so low a temperature the manure does not putrify, and is therefore unsuitable for the plants which vegetate in it. All the morasses appear covered with black, apparently fruitful, turf ; yet the grass can be cut down only every other year, otherwise the strength of the roots would be materially injured. The effect of burning, which is so remarkably beneficial here, proceeds from the increase of temperature which the soil receives, and from the chemical influence of the ashes in producing putrefaction and solution.

The short time that the corn requires for ripening in Lapland applies only to barley and oats. Rye in general requires a longer time to ripen than it does farther south. I have seen rye cut down in Lappmark in the middle of September, which had been sown the preceding year in the middle of July, and therefore had been ten months in the earth before it could be cut down. In *Helsinge-land*, indeed, the rye ripens before the barley, yet the new rye must be sown before the old crop is cut down. In Upsala, after the harvest of the old rye the sowing of the new commences, but not till after the reaping of the barley. At *Calmar*, and in *Blekinge*, on the other hand, so great is the interval between the reaping of the old and the sowing of the new crop, that it leaves more than sufficient time for the complete harvest of the barley. In these places rye ripens three months sooner than in *Lappmark*.

Convallaria majalis is still to be found in sheltered places. *Salix glauca*, on the other hand, climbs up the mountains and spreads itself, as does likewise *salix arenaria*. *Bartsia alpina* appears on the banks of rivers.

Near *Sorsele*, west from the church, at *Raswamyr*, there is a good spring rising in a bason a fathom deep, and apparently screened from the effect of the external air. The temperature at the bottom of the bason among the moving sand was, on the 13th and 24th of July, 35·6°. This cannot be far from the true mean temperature of the earth, as it differs so little from the temperature at *Lycksele*. Another spring, at *Galgalmyr*, near the hill *Swego*

was, on the 19th July, of the temperature 36·68°. The southern plants which have their northern limit at this place are *trifolium repens, plantago major, prunella vulgaris, chrysanthemum inodorum.* *Pedicularis lapponica* appears in the woods, *viola biflora* in the rivulets.

Stora Vindeln ; lat. 65·75°.

This lake is probably situated as high above the level of the sea as *Saggat Träsk* at Quickjock. At Risnaess near a pool I found a spring the temperature of which was, on the 20th and 24th July, 35·34°.

This, and other springs, according to the assertion of the colonists, so cool the pond that it is not possible to remain in it for any length of time; but, on the other hand, the bottom of it is hardly ever frozen. There is likewise a creek behind the buildings of the colonists which never freezes, probably because many strong springs rise up in it. At the edge of this spring grows the *ranunculus lapponicus* in full vigour. It is surprising that so low a temperature should be advantageous to a plant. Deeper in the water grows likewise *tussilago frigida.* *Salix lanata* appears for the first time at *Risnaess.* The same observation applies to *thalictrum alpinum* and *angelica archangelica.* *Carex globularis* disappears, and *rosa majalis* is only found in one other place. Scotch firs still continue abundant at *Stora Vindeln* ; but spruce firs are only to be seen on particular declivities of the mountains. It is more difficult to observe the order in which these trees disappear in the southern parts of Lapland where the land rises rapidly than in the northern parts, where after the disappearing of the spruce firs you pass over a space of ten or twenty Swedish miles before you lose the Scotch firs. On the other hand, the limit of the Scotch firs, when compared with the birches, is so striking, that it has been distinguished even by the Laplanders by a peculiar name, *petsigerk,* or *the end of the Scotch firs.*

At *Givorten-Fiäll* I found a small useful spring at the place of the limit of the Scotch firs, but so elevated that the birches grew only to the height of a man. It is not far from the parsonage of *Tärna Capele,* and is called by the Laplanders *Giltingen-ajek.* Its temperature was, on the 22d July, 34·52°.

From the temperature and vegetation, the climate here is similar to that of *Vadsöe,* at Wardöehuus, in Norwegian *Finmark.*

Givorten-fiäll is the first mountain range at which we arrive in Umeo Lappmark. It constitutes a long ridge between *Juckt-Träsk* and the river *Vindel,* above the lake *Stora Vindel.* Its height scarcely reaches to half the distance between the line of birches and the snow line. Flakes of snow lie nowhere upon these mountains, except upon an insulated peak, which on that account is called by the Laplanders *Kjallemene Nase,* or *cold nose.* Even upon it only very small spots of snow are to be seen. Below this peak, and higher than *Giltingenkälla,* there is a good spring, distinguished by

the name of *Nase Gauen Ajek* (*nose valley spring*). Its temperature, on the 22d of July, was 34·16°.

A third spring, *Valetjocks-källa*, lies directly above *Giltingen-källa*, on the north side of the little hill *Valletjock*. Its temperature, on the 22d of July, was 33·8°.

The Laplanders are well acquainted with all the springs in these parts. They supply them with water when all the rivulets and rivers are covered with ice. There is likewise a spring on the south side of the ridge, called *Praesten Middags Källa*, about a Swedish mile south-west from the preceding, and a quarter of a mile north from *Ammor Träsk*. Its temperature, on the 22d of July, was likewise 33·8°. *Andromeda hypnoides* was the only plant which covered the ridge of the mountain.

Near the snow line we can hardly expect any other temperature of the earth than the freezing point; for supposing the temperature of the air to be higher, how could it counterbalance the perpetual oozing of the melted snow? especially as it is the water which spreads and equalizes the temperature of the earth.

IV.

Observations on the Heat of Springs in the Southern Provinces of Sweden. By George Wahlenberg.

I have continued my observations on the heat of springs at Upsala, chiefly because I was of opinion that the temperature of inconstant springs might give the difference between the mean temperature of different years and seasons. Nor have my hopes been entirely disappointed, as the following observations will show:

	Spring at Lassbybackar.	Professor Spring at Haga.
1810— 4 November	42·08°	45·14°
14	41·00	44·24
24	39·2	——
26	——	43·43
13 December	37·76	42·26
1811—17 January	36·14	——
31	35·96	39·83
4 March	34·36	——
16	34·16	38·57
2 April	33·89	38·48
17	33·62	——
24	33·80	38·48
29	34·07	——
3 May	34·88	——
13	38·12	38·75
19	43·52	39·00

Both springs were colder throughout this winter than the preceding. The first as much as 0·72°. This is the more remarkable,

as the winter 1810, 1811, was distinguished by the mildness of its temperature; and in this respect far surpassed the winter 1809, 1810. Perhaps the lower temperature of the springs was owing to the cold summer of 1810.

But this difference is not sufficiently satisfactory, as it leads to the expectation that it would have produced an effect likewise upon the constant springs. I found in 1811 the temperature of

Langgghjördes upper spring at Haga, almost as in the preceding year, April 24		42·08°
Gnesta mill spring { March 27		42·26
{ April 26		42·26
Norby spring, April 24		42·71
Norbyland spring, April 24		42·26
Myrby spring, April 26		43·70
Priest spring at *Haga*, April 24		43·34
Upsala mill spring, April 29		43·61
Brennery, or Sandviks spring { March 20		43·70
{ May 3		43·70

From this table it appears that the constant springs do not alter their temperature, or that the alteration is very insignificant. But the heat of the inconstant springs was uncommonly great in the spring, almost in the same proportion as the spring was distinguished by its unusual mildness.

*   *   *   *   *   *

I have had an opportunity of making the following observations on the temperature of springs in the southern provinces of Sweden.

On the north side of the little hill called *Blacksta*, lying a little to the south of *Langstakrog*, in *Sudermannland*, there is a strong spring. On the 5th of September, 1810, its temperature was 44·42°, which agrees very well with the presumed temperature of the earth in that part of Sweden.

Norrby Sauerbrunn, half a Swedish mile south from *Swärdbro*, in *Sudermannland*, rises with a strong current. Its temperature was as follows:—

1810— 6 September.............. 44·06°
1811—26 May .... 43·07

Not far from this, at *Ladugard*, there rises another mineral spring, the temperature of which was as follows :—

1810— 6 September .... 44·24°
1811—26 May .... 43·16

Himmelstarlunds Sauerbrunn, at Norrköping, a good and strong spring :—

1810— 7 September .... 44·24°
1811—27 May .... 42·8

The changes of temperature happen to coincide with those in the upper *Langhjerdes* spring at Upsala. Hence the mean temperature of the earth here is probably 44·6°.

The mineral spring of *Suderköping*, called *Ragnilds-källa*, at the south gate of the town, is the finest spring of the kind that I have seen in the southern provinces. The water rushes out with such impetuosity that it is not possible to place the thermometer in the bason itself. I found its temperature as follows:—

1810— 8 September.............. 45·86°
1811—27 May .... 45·86

Such an agreement puts the mean temperature of the earth in this place out of doubt. It may appear high when compared with that of the preceding springs; but the observations at Upsala have shown us that the highest temperature always belongs to the constant springs. Of plants I may notice the walnut, which frequently grows here to a large tree and bears fruit; which, I know well, is not the case farther north.

A spring in a rivulet at *Knapphulla*, in the northern part of the county of *Calmar*, gave, on the 9th Sept. 1810, 45·32°.

The small spring at the Inn of *Lund*, near the town of *Westerwick*, gave a more inconstant and smaller result. It was—

1810—10 September .... 44·6°
          17 Ditto .... 44·6
1811—29 May .... 42·44

On the other hand, Wärnaby spring gave a still higher estimate. It lies the eighth part of a Swedish mile west from the country-house of *Wärnanäs*, in *Södermöre*. The bason of the spring is almost two fathoms deep, and the quantity of water which it yields is always equally great. Its temperature was—

1810—13 September.............. 46·76°
1811— 1 June .... 46·4

Here the mean temperature of the earth may be estimated at from 46·76° to 47·12°; an estimate which is the more important, because it is here that we must place the northern limit of the beeches. We find indeed some beech bushes farther north, as at Wongelslät, in the parish of *Rysby*; but the first beech woods make their appearance at *Wärnanäs*, and there only on the level of the sea. Farther inland the tree, though planted, does not thrive. There are no beech woods in the interior till we come to lämgö,i *Bleckinge*.

My endeavours to obtain a greater number of good observations near the line of the beeches were not successful. Near the meadow of *Norregards*, in the parish of *Suderokra*, there is a strong spring, the temperature of which was as follows :—

1810—15 September .... 46·04°
1811— 1 June .... 44·42

Somewhat less than *Wärnaby* spring.
Another spring, at *Bruntorp*, was—

1810—14 September .... 47·48°
1811— 1 June .... 44·6

The variation was smaller than that of *Professor spring* at Upsala, but greater than that of *Langhjerdes* spring. Hence it is probable that the mean temperature towards the 20th of August would be 46·94°, which agrees with that of Wärnaby spring. Notwithstanding the uncertainty of such an estimate, it serves at least to show that this country has the advantage over Upsala in point of temperature.

The temperature of the Admiralty spring of *Lyckeby*, three quarters of a Swedish mile north from Carlscrona, was as follows :

1810—15 September .... 47·84°
1811— 1 June .... 45·32

If we assume the preceding data, these numbers give us 47·3° for the mean temperature of the earth in this place.

In *Schonen* I examined a spring at the church of *Maglahem*. Its temperature was—1811, 4th June, 44·6°.

I likewise examined the *Raflunda* spring, which Linnæus mentions in his journey through *Schonen*. Its temperature was, on the 4th June, 1811, 44·96°.

Neither of these observations is sufficient to enable us to determine the mean temperature of the earth in *Schonen*.

V.

At Berlin I have only found one spring convenient for such observations; but it appears extremely well adapted for them. It is the perennial mineral spring in the *Panckow*, a quarter of a mile north from Berlin. Its temperature was as follows :—

1811—27 August .. 49·46°   1812— 9 January .. 49·46°
    7 September 49·46   13 February . 49·10
    29 .... 49·46   14 March ... 49·10
    9 October .. 49·46   1 April .... 48·92
    7 December 49·46

VI.—*Result.*

In all northern countries the mean temperature of the earth is higher than that of the air; and the difference appears to increase as we advance northward, or as the cold of the winter becomes more severe. The following table shows the rate at which the temperature of the earth varies according to the latitude.

Plate III.

Geognostic View of the Mountains of Ancient Latium.

Geognostic View of the Mountains

Sphæra Plani

Planities Romana

Lucus Liquili

Promontorium

Elysium

Lycus Phrenitis

Scmen album
(to the subsurface)

Peperin
Lava Species
Compact Lava
Volcanic Earth
Scoria
Submarine Mountains

Places.	Height above the sea.	Latitude.	Temp. of the earth.	Temp. of the air.
	Eng. feet.			
Berlin	127·6	52·5°	49·28°	
Carlscrona		56·25	47·3	46·04°*
Warnabykälla, line of beeches		57·5	46·76	
Söderköping		58·5	45·86	
Lägstakrog, Sudermannland		59·	44·42	
Lake Yngen, Vermeland	549·9	59·5	41·00	
Upsala		60·	43·70	42·03†
Dalelfmündung, line of oaks............		60·5	42·26	
Geffl.		60·75	41·9	
Huddikswall		61·75	40·64	
Sundswall		62·5	39·2	
Umeo................................		64·	37·22	33·38‡
Degerforss		64·25	36·68	
Lycksele	639·4	64·5	35·60	
Stora-Windeln	1129·7	6 ·75	35·24	
Line of birches at Giworten fiäll	1705·2		34·16	

Article VII.

Some Account of the Mountains of ancient Latium, in which the Mineral called Haüyne is found. By Leopold Gmelin, M. D. §

(With a Map.)

Haüyne is found partly in the volcanic tuff called *peperino,* and partly in the soil that covers it.

Peperino is a soft fragile stone, very fit for building, of an earthy texture, and not heavy. It seems to consist of a congeries of very different bodies, particularly fragments of augite of a dirty green, dark green mica, iron sand, compact limestone, basalt, and a kind of lava very much resembling pumice. These seem to be agglutinated by an earthy cement. Sometimes it contains fragments of stones.

These fragments are always sharp-edged, generally small, but sometimes weighing several pounds, especially those consisting of basalt and limestone.

Sometimes, though rarely, fragments of felspar, and a scoreaceous matter of a dark green colour are mixed with the peperino. But as far as I know it never contains melanite.

What I regard of considerable importance with respect to the

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* In Lund, according to Nenzelius and Scheumacher.
† According to the observations kept for 30 years at the Observatory.
‡ According to the result of five years' observations by Dr. Naezen.
§ Abstracted from his Thesis lately published at Heidelberg.

origin of peperino is, that it often contains pieces of charcoal,* which cannot be distinguised by the vulgar. It contains likewise wood not charred, but rendered pale and similar in appearance to amianthus.

From what I have said, it is obvious that peperino is composed of fragments of very different minerals, agglutinated at a time when this part of the earth was covered with trees. Whence these fragments and the cement came will appear as we proceed.

Peperino, along with a peculiar spongy lava, which the Romans call *sperone*, and with a little compact basaltic lava, which they denominate *selce*, forms the congeries of mountains that constitute Old Latium, and which, almost touching the Appenines at Præneste, separate the plain of Rome from the Pontine marshes.

This porous lava, the mass of which contains a prodigious number of small opaque leucites, many augites, and some mica, is perforated by a great number of long irregular pores, the surface of which is unequal and dull. This lava sperone constitutes the greatest part of these mountains, and particularly all the Tusculan mountains, from Tusculum (called at present *Frascati*), as far as Corbium (at present denominated *Rocca Priora*), mount Albanum, the highest of all (*monte Lazio* or *Cavo*), and the rock in which the village called *Rocca di Papa* is situated. If any inference can be drawn from external form, and from their connection with the others, mount Algidus, likewise parallel to the Tusculan mountains; mount Artemisius, lying towards Velitræ; the mountains Ariani; and the mountain called *La Fajola*, are composed of the same lava.

The peperino, beginning at the bottom of mount Albanus and Artemisius, between south and west, surrounding the lakes *Albanus* (*lago d'Albano*), and Nemorensis (*lago di Nemi*), with a high and steep border, gradually sinks into the plain of Rome towards the sea and Velitræ. But besides these lakes, there is a valley particularly worthy of attention, called *Valle Riccia*, situated below the village Aricia. This valley has an oblong bason form shape, and is surrounded with steep rocks, except towards the sea. It is said to have formerly constituted a lake.

The mountains composed of *sperone* differ from those consisting of *peperino*, chiefly in forming high and long ridges, very steep, and without any traces of a crater. The peperino hills are not so high, broader, slope gently, and in the centre exhibit a lake or concavity, which may be conceived as the remains of a former crater.

The compact lava, which is seldom entirely free from pores, when it contains them in abundance becomes exactly similar to the sperone. Its colour is usually grey. It contains always augites, almost always leucites, often mica, seldom felspar, and never

* Riccioli, a dealer in minerals in Rome, has a specimen of charcoal in peperino an inch thick, and nearly five inches long.

olivine, as far as I myself have observed. It is split into irregular masses, and occurs in many places, though it never constitutes entire hills. Small rivulets of it are seen near Rocca di Papa, towards the chapel, called *Madonna del Tufo.* It occurs also scattered over the whole summit of the Tusculan mountains, particularly on the rock on which Rocca Priora is built; but the greatest masses of it occur at the bottom of the Tusculan mountains, constituting oblong hills, which run out into the Roman plain.

Nor is the region in which the peperino prevails without a portion of compact lava. For it is not only found at the foot of the hill which looks towards the Roman plain, as at the *Grotta Ferrata,* and the place called Alle Fratocchie, below the city of Albano; but what is surprising, it covers in several places cliffs composed of peperino, which surround the lakes Albanus and Nemorensis, constituting rocks which rise perpendicularly from the shore of the lakes to the margin of the crater, so that one would be tempted to believe that the whole crater is formed of lava, over which the peperino has been deposited. The lake Nemorensis is not only almost surrounded by compact lava, but at the part lying below Nemi, and mount Albanus, it is decorated by rocks composed of sperone, through which the fountain of the nymph Egeria runs.

The peperino and compact lava are seldom immediately under the surface of the ground. They are commonly covered with other volcanic products. The lava sperone, the nearer the surface it is, becomes not only more porous and soft, but composed of distinct fragments about the size of a nutmeg or walnut, conglutinated together, and forming beds generally parallel to the declivity of the mountain, as may be very well seen at the Rocca di Papa. At length it acquires the aspect of pumice; but differs from it in having nothing fibrous in its structure. These scoriaceous beds may be observed at the chapel of *Madonna del Tufo,* near the lake Nemi, and in the Tusculan mountains. They are frequently covered with a fat reddish earth.

The compact lava is also frequently mixed with a reddish earth, not so fat in its feel as the preceding. It is employed in the making of cement.

Leopold von Buch has, in an excellent book,* given the name *wacke* to the rock of the Monti Verdi, and of the Aventine and Capitoline hills, which must certainly be considered as a volcanic tuff, and as very similar to the volcanic tuff of Naples, only a little harder. Yet the earthy texture, and the mixture of reddish pumice, sufficiently distinguish it from that mineral. Under the term volcanic tuff, he designates very different substances found in the plain of Rome : namely, fragments of pumice, puzzuolana, and the common volcanic earth, ashes, and sabbia. All these

* Geognostiche Beobachtungen auf Reisen durch Deutchland und Italien. Two vols. 1809.

bodies, according to him, have been washed from the mountains of ancient Latium, and have been more or less agglutinated together.

But as no pumice has hitherto been observed in the mountains of ancient Latium, neither these beds, nor the mountains Aventine and the Capitol, and the monti Verdi, can have drawn their origin from these mountains. The tuff of the Monti Verdi, and of the Aventine and Capitol, must rather be reckoned among the most ancient volcanic products of this country, thrown out in the very places where it is now found, and therefore traces of it are to be seen no where else. But in consequence of the great lapse of time since it was produced, it is too much shattered and covered with other minerals brought hither by water, to enable us to find any traces of a crater. Hence, though I admit, with my friend Breislak, that this tuff was thrown out of a crater not far from Rome, I am not of opinion that any thing certain can be pointed out respecting its exact situation.

As to the common volcanic earth, which abounds in fragments of lava and in leucites most usually decomposed; and the puzzuolana which always lies under the common, although their spreading out into beds that cover the whole plain of Rome, may seem to prove that they have been deposited from water; yet I cannot admit them to be derived from the mountains of old Latium, which indeed are sometimes covered with a little earth, but generally of too fatty a nature to be employed for making a cement, and at the same time containing very few leucites. Hence, I do not consider it as absurd to suppose, that the lakes Gabinus and Regillus were formerly craters which threw out lava, afterwards spread by the agency of water over the whole plain of Rome. Probably there were even more craters in this plain which are now entirely obliterated. One of them perhaps threw out the melanites which are found at the bottoms of so many mountains of ancient Latium. But the formation of the common volcanic earth and the puzzuolana seems to have been the most recent of all, not only as it covers all the bottoms of the mountains, but as it in some places alternates with beds of travertino.

The peperino, in that part which lies towards mount Albanus, is covered with the same scoriæ as the lava sperone, and no doubt with that lava itself. In other parts it is merely covered with an earthy crust which seems derived from its own decomposition. As it is not capable of resisting the action of the weather, its surface is split into soft crusts which are continually going to decay.

The peperino is divided into distinct beds, from a few feet to more than a hundred in thickness. These may be seen very well on the west side of the hill on which the town Marino stands. In that place beds of harder and softer peperino appear to alternate.

These beds appear to ascend from all quarters towards the lakes which I have mentioned above. They are almost parallel to the declivity of the hill; but in that part where the lake Albanus is,

they descend a little towards the lake. This I observed above the cross at Nemi, and less distinctly above the monastery Palazzuola, in that bed in which the consular fasces are engraved.

It deserves attention that, on my way to the town Albano, not far from the place where the Via Appia joins the present road, I observed two opposite stratifications. The lower beds ascended towards mount Albanus, and were cut off and covered by the upper bed which rises towards the sea.

I never observed the lava sperone stratified, although the rocks of it are cut perpendicularly in the first Tusculan mount, from which stones were taken to build the theatre and other houses. The scoreaceous lava lying over it is divided into beds, as I have already observed, usually parallel to the declivity of the mountain.

I shall now notice only what appears to me most probable respecting the formation of these mountains.

The peperino and volcanic tuff, from which it differs chiefly in containing, instead of pumice, a porous clayey lava which is not fibrous, and in containing numerous calcareous fragments, seem to be ashes thrown out of a volcano, and afterwards conglutinated. For under the improper name of *ashes* we may comprehend every thing thrown out of a volcano in the form of powder. Whether this powder be the real ashes of the fuel which feeds the volcano, or only a mineral near the fire and by its agency reduced to powder; whether the numerous crystals of augite and mica were formed by the fire, or previously existed in the rocks acted on by the volcano; whether the numerous fragments of basalt in the peperino had a Neptunian origin, or are to be considered as pieces of ancient lava, solidified under the earth, and thrown out in the form of fragments by the agency of new heat: these are questions which cannot be certainly decided from our present knowledge of volcanoes. It seems more certain, that the limestone has not been formed by the agency of fire, but only broken from the volcanic mountain, and thrown out little altered. I think the same is the case with the fragments of compound stones above mentioned, especially those that contain haüyne; although the mica and augite contained in them indicate a certain relation to volcanic bodies.

The craters, from which the peperino was thrown in the form of powder, are the lakes Albanus and Nemi, and the valley of Aricia. This not only appears from the situation of these lakes in the centre and highest part of the country, but likewise from this circumstance, that all the beds of peperino (those excepted which lie towards mount Albanus) rise towards these lakes.

It is not surprising that the same matter should be thrown out of different craters; for as long as the volcanic fire acts upon the same materials, it must produce the same bodies: nor can the crater at which this matter issues make any difference. Thus for example, the Phlegraean field near Naples is composed of the same tuff, though we know that it issued from a great variety of craters; nor can the tuff of the promontory of Misenum be distinguished

from that of mount Gaurus. Thus it appears that Vesuvius, in different eruptions, some from the summit and some from the sides, produced exactly the same substance.

It seems clear that the peperino extended as far towards that quarter which is now occupied by mount Albanus and the others, as it does towards the other quarters.

That the eruptions of peperino took place at very different times, is evident from the alternation of hard and soft beds of it, and from that opposition of beds which I observed at the town of Albano.

There is no proof that the sea covering these regions occasioned the conglutination of the peperino, and then retired to its present bed. For no marine bodies can be found either in the peperino or over it; while the charcoal and wood found in it, seem to prove that these regions, before the volcanic eruptions, were covered with trees and shrubs like the Pontine marshes.

The conglutination may have been effected by the rain-water alone, which, passing through, filled up the intervals with powder, and dissolving a portion of the carbonate of lime (assisted by the carbonic acid of the atmosphere), deposited it again to serve the purpose of a cement. I found that all the fragments of peperino, even those in which no limestone could be perceived, effervesced in acids, and that not merely in particular places, but through their whole mass. In this respect peperino differs from volcanic tuff, which seems rather cemented by a silicious matter, and may be compared to the ashes above Herculaneum, which, being moistened by water containing lime in solution, effervesce when mixed with an acid. Yet calcareous matter must not be considered as the only cement of peperino; for some of its beds have rather an earthy than a stony consistency; and as the composition of the peperino itself differs, it is reasonable to infer the same difference in its cement.

Perhaps, after the emission of the powder which constitutes peperino, the volcanic fire attacked other materials, which it liquefied and projected through various openings. Hence, probably, the origin of all the mountains composed of sperone and basalt. As no stratification is perceptible, we may conclude that the whole was thrown out at once.

It would be in vain to look for the craters from which this fluid matter issued; because as soon as the energy which throws out these fluid matters ceases, the craters themselves being filled with fluid matter, it solidifies, fills them up, and no depression can be perceived in the place where the crater was situated. That the fluid matter of which these mountains are composed speedily becomes solid, is evident from their steepness; for fluid lava would rather have spread wide than formed such precipices.

When metallic matters are in fusion we know that the heavy metal sinks to the bottom, while the lighter scoriæ float upon the surface. In like manner the compact lava is almost every where

covered by the porous. The former is visible but seldom on the higher parts of mountains and only in small streams; but at the bottoms of the mountains we find it in greater abundance constituting hills. Over the lava sperone lie beds composed of fragments of the same lava, conglutinated together; over them lie beds of scoriæ, or fragments of sperone, more porous and vitrified, and not conglutinated together; over the whole is deposited a dark red fatty clay, which seems to owe its origin to the ashes thrown out towards the end of the eruption.

The beds of peperino descend towards the lake on that side which looks towards mount Albanus, because they were lifted out of their original position by the strength of the fire which threw out the lava of mount Albanus.

I could no where see distinctly the lava of mount Albanus lying over the peperino; but you can see thick beds of scoriæ, doubtless produced at the same time with the lava, covering the peperino above the lake Nemi. In the same manner, not far from the margin of the lake Albanus, above the monastery called Palazzuola, the scoriæ of mount Albanus begin to predominate, and the lava itself succeeds them towards the Rocca di Papa. If you suppose the peperino formed after the lava, it is impossible to explain why we can find no traces of peperino or ashes on mount Albanus or the Tusculan mounts, though these mountains are so near the craters of the peperino, that they may be said to rise from the very margin of them. Von Buch, indeed, observed basalt lying under the peperino, in a valley near the fountain called Dell Aqua Tepidula. I did not see the place. But as the lava, though formed after the peperino, may have passed through its fissures, it may no doubt be seen lying under it, though of later formation.

The origin of the compact lava surrounding the walls of peperino, and in many places covering it, is of more difficult explanation. Two opinions respecting it may be entertained. Either the craters constituting at present the two lakes, after they had thrown out the peperino, became also filled with lava which in some parts adhered to the walls of peperino, and when the volcanic energy was at an end falling partly back again into the abyss, left an opening as a receptacle for waters. Or all this lava flowing down from a higher crater, and particularly from mount Albanus, adhered to the walls of the peperino. What makes against this latter opinion is, that the lava surrounds the lake even in the parts most remote from mount Albanus. Thus the lake Albanus is surrounded with great rocks of lava between Castle Gandolfo and the town Albano. The same remark applies to the lake Nemi, on its south side, between the towns of Gensano and Nemi. Nor in these places is any lava to be seen lying above the peperino.

On that account, I am disposed to believe that the compact lava came from the same crater with the peperino. I conceive that only the lava sperone, which forms a rock below Nemi, flowed from mount Albanus, as the scoriæ of that mountain extend as far as the margin of the lake. To confirm this opinion, it would be

necessary to prove that all the lava surrounding these lakes is of the same nature, but different from the compact lava of mount Albanus. A few observations which I made seem to show this. The lava of the two lakes is very compact, and usually destitute of those pores which distinguish the lava of mount Albanus, so that it cannot be distinguished from the most compact basalt. The lava of the lake Albanus only contains large leucites; but that of mount Albanus contains likewise innumerable small ones. The lava at the west gate of Nemi is destitute of leucites, and contains instead of them small crystals of felspar. But no such crystals can be seen in the lava of mount Albanus. These observations I admit are too few to determine the point. It would be necessary to institute a minute comparison between the lava surrounding the lakes, and that of mount Albanus.

The stone containing haüyne is found in the whole peperino, and often in the earthy soil seemingly formed by the destruction of peperino. Thus it is found at Nemi, Gensano, Albano, near Tusculum, and at Rocca di Papa.

But it is for the most part collected in the great peperino quarries below the town Marino. From this place I obtained the specimens which I subjected to examination.

Haüyne, dark green mica, augite both crystallized and in grains, a white mineral, and oxide of iron, mixed in various proportions, constitute this stone. Sometimes the white mineral is wanting, the proportion of mica is greatly increased, and the haüyne acquires a green colour. Sometimes mere dots of blue haüyne are to be observed scattered through the white mineral, and in that case the mica is very scanty. The proportion of augite is usually small, and its particles are often so minute as scarcely to be determined by the naked eye. The oxide of iron is less abundant than the augite. It is often partially converted into red oxide of iron.

Article VIII.

Memoir on the most proper Means of accelerating the progress of Mineralogy. By M. T. C. Braun Neergaard. Read to the First Class of the French Institute in Nov. 1812.

THE question has been often asked of me, whether, after having seen so many minerals during my travels, and being in possession of a collection as excellent for the choice of the specimens as instructive in consequence of the numberless suites which it contains, I had any intention of ever publishing a *System of Mineralogy?* My answer has always been in the negative. It was not dictated by that false modesty, which often injures the progress of science, and which would not have blinded me so far as to decline a career so flattering to my

vanity, by placing my name side by side with those who have employed themselves so successfully in promoting the science which I cultivate. Nothing would have prevented me from giving a system of mineralogy if I thought I could have composed a better than the best which we have at present. I should have considered it as a sacred duty to facilitate the study of a science, the difficulty of which often disgusts the beginner, and even stops the career of him who had flattered himself that he would be able to extend its boundaries. Mineralogy has for some years past constituted the most agreeable moments of my life. The more I know of it the more I desire to know.

Every day new minerals become known to us. Though the last age was rich in discoveries, nature is far from being exhausted. Our successors and followers will find sufficient employment for their talents; and perhaps will not be very grateful to us for the facilities which we have procured them, any more than we are towards those who have opened to us the career of the science. If this great variety and number of objects becomes agreeable to the *amateur* by the interest which each new mineral inspires; at the same time that the number of genera and species augment, it becomes more and more difficult to reduce them under any regular method. I hope this difficulty will soon be felt, and that the systems of an Haüy and a Werner will at last set bounds to the prevalent fury of making every thing anew.

Those who cultivate mineralogy with success will find enough to satisfy their ambition, without multiplying the number of systems. This beautiful part of natural history, which its difficulty alone obliged naturalists so long to neglect, leaves much to be filled up in its details. I have often smiled when I heard beginners in ecstasy at the perfection of the science, and some years after recount to me the difficulties which retarded them at every step. It is unfortunate that mineralogy did not find its Linnæus and its Buffon at the same time with botany and zoology. They have come at last: we have seen them appear. They will leave to our successors philosophical principles according to which they may direct their researches.

The traveller, for it is always to him that the philosopher who studies in his cabinet owes his materials, will still find a sufficient number of objects to elucidate. The study of geognosy will teach the traveller how useful it would be to unite more closely these two sisters, who ought always to be inseparable. It is his province to make us acquainted with new substances, new positions, new localities. My friend M. Leonhard, who is so much employed in promoting mineralogy, has proved, by his work entitled *Topographie Mineralogique*, how many difficulties this subject presents. He has done a real service to all the lovers of this part of the subject, by collecting all the scattered materials which he could find; and his laborious work will facilitate the researches of those who wish to fill up the numerous blanks

which still remain. We cannot always depend upon the information of dealers in minerals. Interest often induces them to substitute one country for another. Neither can we, in all cases, make out the localities from the specimens which we find in the cabinets of our friends. The rock, and other characteristic marks, are often common to different localities. I may, out of numerous examples which might be chosen, give one from my own collection. It is a specimen of *axinite*, from the fountain of La Caille, in the valley of Chamonix, which has been often conceived to have come from Dauphiné. It is likewise of importance to make known the different substances which accompany the same mineral. Nothing contributes more to make us acquainted with the country to which a substance belongs, because the same substances very generally associate together. The eye of the traveller accustomed to observe often rectifies errors by seeing substances in their place, and by examining the depots from which cabinet specimens have been taken.

In reality our greatest specimens are only atoms in comparison with the masses which we are accustomed to observe in nature. The rocks which we collect for our geognostic collections, are of little importance, except when they have been selected on the spots by ourselves or our friends. These fragments serve for the commencement of the study of geognosy in our cabinets, and they bring to our recollection the great revolutions which the globe has undergone. These mountains always fill us with admiration of the Almighty, to whose goodness we are indebted for every thing. When false localities are assigned to rocks it almost always produces serious errors respecting the localities of mountains.

It is easy to see that the geognost cannot really promote his science except by travelling. We ought likewise to employ ourselves in such researches as enable us to determine whether any particular substance is new or not. A new name given to a substance often induces people to believe that the substance is new. This error proceeds from the little exactness in the older descriptions, from the small progress that analytical chemistry had made, and from the little attention which celebrated men, such as Buffon and Linnæus, formerly paid to mineralogy. Substances, in which we have at present found a sufficient number of distinct characters to separate into peculiar species, were formerly confounded together under the same name. This kind of indication will not only be useful to the historian of mineralogy, but it will present likewise to amateurs the facility of finding rare substances in old collections placed among common species. The *ichthyophthalmite* of Andrada, to which Haüy has given the name of *apophyllite*, was known not only by the name of *zeolite of Hellesta* in West Gothland, but likewise under that of *zeolite of Uto* in Sudermanland. I have found very fine specimens of

apophyllite under this last name in the collection of Secretary
Brandt, a collection unique for old substances from the North,
which I purchased at Copenhagen some years ago. I even had a
keen discussion respecting this substance with M. Engeström, who
insisted that it was a zeolite. This philosopher published an English
translation of Cronstedt's Mineralogy. He has likewise published a
good mineralogical guide through Sweden. All this takes nothing
from the merit of Andrada, who first made the substance known,
with characters sufficiently distinct to constitute a peculiar
species; nor of the celebrated Haüy, who first determined its
crystallization. I hope one day to publish some details about
this rare mineral, which will serve to explain its position, and
the minerals that accompany it. These observations are the
fruits of my journey through Sweden, and of the researches
which I made on the spot. I likewise found in the same cabinet
a splendid specimen of *botryolite*, under the name of *opake
mamellar chalcedony*, which the possessor considered as of so
little value that he had placed it among his duplicates. The
botryolite, since the substances from Arendal have attracted the
attention of naturalists, has likewise borne the name of *reddish
manganese*, till Klaproth discovered in it *silicious borate of lime*.
It was this discovery that induced Haüy to make it a variety of
the species to which the Germans have given the name of datho-
lite, a species discovered by my countryman Esmark.

Those persons who choose to occupy themselves with crystal-
lography will likewise find sufficient materials to employ their
sagacity. Nature, though it acts after fixed principles, is not-
withstanding inexhaustible. By observing and comparing we
shall continually find new varieties of form. We shall even
sometimes have the good fortune to discover the primitive form
of a substance, and thus prove by inspection what our illustrious
master, Haüy, predicts from calculation.

The mineralogist even sometimes gets the start of the chemist
in the knowledge of the constituents of a mineral. He predicts
from the crystalline form alone, and before he begins his experi-
ment, what the result of it ought to be. He is induced to
recommence his analysis if his first attempt does not accord with
his prediction, and a more careful experiment at last brings the
chemist and mineralogist to agree.

Chemistry alone, in my opinion, cannot serve as a basis for a
mineralogical system; but this does not prevent me from ac-
knowledging the great advantages which mineralogy has drawn
from that science, and which she will continue always to draw
if she employ chemistry with prudence. Mineralogy has ad-
vanced only as the art of analysing minerals advanced.

The great progress of chemistry began in the North. The
names of Bergman and Scheele will always be placed at the head
of the philosophers who have changed the face of the science; a
science without the aid of which many manufactures would have

remained in a state of infancy, a science which every day advances the progress of medicine. The principles according to which these philosophers directed their labours have now altered. Their discoveries, in consequence, may lose some of their utility, and may excite less interest; but they will not, on that account, lose their merit. These chemists cannot, and ought not, to be judged but according to the knowledge of the period when they lived. A man ought to be acquainted with the progress which the science which he cultivates has made, both in his own and in other countries; but we ought not to require of a philosopher to penetrate into the progress of future ages. The name of Lavoisier will be always dear to those who know the difficulties he had to overcome in order to accomplish his object. Few persons are possessed of a spirit of philosophy so strong as to enable them to struggle against opinion generally received. It is always dangerous to take a part against opinions to which time and experience have affixed the seal of truth. Those obstacles which would have often been insurmountable to another did not prevent Lavoisier from opening a new road in chemistry. Davy, that English chemist who has drawn upon him the attention of his brother philosophers in France, will he not force us by degrees to believe facts which experience perhaps will one day confirm, though the principles at present adopted are often at variance with the views of that celebrated philosopher?

The analyses of minerals are very difficult to make. Hence the reason why so few are exact. Klaproth and Vauquelin have brought the art to a state of great perfection. They have brought their analyses to an uncommon degree of exactness, which it would be difficult to surpass. Rose, Bucholz, Laugier, Descotiles, Berzelius, Ekeberg, and others, have skilfully followed their footsteps. Vauquelin and Klaproth often make discoveries at the same time: it seldom happens that they do not agree when they operate upon specimens from the same place. Exactness alone is not sufficient in an analysis. The least portion of matter foreign to the body subjected to analysis, whether it be the containing rock, or any other mineral, occasions an error in the chemical result. Nature, while at work in her great laboratory, has often agents at her command that are unknown to us. The manner in which decomposition is produced will perhaps always remain a secret to us; but it is impossible to doubt that the nature of a mineral ought to change as soon as its external characters become quite different from what they were before.

No system is easily constructed when the object is to construct it well. Systems of mineralogy will be always more difficult than those of the other branches of natural history. The nature of the objects which it includes opposes itself to its perfection. In mineralogy we have not the advantage, as in zoology, of subjecting living beings to rules, nor, as in botany, plants which in dying reproduce their genera and species.

, It is not, therefore, in the power of all the world to construct a system of mineralogy; but it is not so difficult to make corrections in those systems which exist already. A mind prone to philosophize in excess sometimes discovers faults, when he ought rather to admire the genius of him who has been able to avoid them. The small spots which the searcher after faults pretends to find out, are not so much errors in him whom he accuses, as consequences of his own erroneous manner of seeing. A reputation justly deserved by long labours, by profound studies, and difficult researches, is sometimes disagreeable to the man who despairs of ever being able to reach it. He seeks, therefore, to destroy it; but the factitious means which he is obliged to employ serve rather to produce an effect quite contrary to what he intended. He gives additional force to his adversary, instead of weakening him. His criticism induces us to read over again and study the work, and thus, by understanding more completely, we form a higher opinion of its value. An edifice built upon a solid foundation is not easily destroyed.

A system of mineralogy is not only necessary for him who commences the study of that science, but likewise for the arrangement of cabinets. Without some system it would be difficult to find in our cabinets such a multiplicity of new substances as have been brought together within these twenty years.

To make mineralogical collections classed according to the country from which they come, a method still employed in Sweden, can never enable us to learn mineralogy. Such collections can never be methodical, because the same substances do not occur in every country; even if they did, such an arrangement would occasion perpetual repetitions, and oblige us to have an immense number of specimens. Geographical collections can only be useful along with systematic ones. In order to have them tolerably complete we must confine ourselves to our own country. We see such a collection in the School of Mines of France. The zeal of its directors have formed it ; and the care of its keeper, my friend M. Tonnelier, has given it an order that may enable it to serve as a model for similar collections in other countries of Europe. The easy access which this estimable philosopher gives to all the friends of the sciences renders this collection as useful as it is precious.

It is happy for the age in which we live that men such as Werner and Haüy have chosen to occupy themselves with the formation of mineralogical systems. These two celebrated philosophers have not constructed their systems according to the same principles; but this does not prevent both systems from being employed with success. They may guide us in the study of mineralogy. They may facilitate the arrangement of our collections. The use which the student makes of them will assist the progress of a science difficult in its nature, but which their penetration has been happy enough to facilitate.

Posterity will give the name of philosophic mineralogists to
Haüy and Werner. The name of philosophical geognost will
be given to Werner, the creator of geognosy; and posterity will
not refuse a place by his side to the companion of my travels,
the immortal Dolomieu.

It is difficult to establish a good mineralogical nomenclature ;
because the name given to a mineral often occasions false ideas,
and makes us ascribe to it exclusive characters which it never
possessed. We are greatly obliged to the philosophic Haüy,
whose friendship does me as much honour as it has been useful
to me, and to other distinguished mineralogists who have re-
cently endeavoured to banish from the science as much as pos-
sible all names drawn in a great measure from localities and
colours. When the name is drawn from the locality we are
induced to suppose that the mineral exists only in the place or
country indicated by the name. We find the same substance in
different places, and then the name no longer applies. The
mineral called *Haüyne* got the name of *Latiolite*, because it had
been discovered in the mountains of *Latium*, and because it was
conceived that it existed nowhere else. Time has already proved
the contrary, for it has been found in Vesuvius. It is even pro-
bable that the borders of the Rhine, and the mountains of Au-
vergne will one day claim the privilege of being classed among
the localities of a substance, the name of which seems to have
inspired the interest at present bestowed upon it.

Names drawn from colour are still worse. The infinite number
of shades which accompany the different colours, obliges us
often to search in vain among a great number of specimens for
the colour from which the name is derived. It is so easy to find
an example that I do not think it necessary to cite one. Almost
all the names drawn from colour might serve to confirm the
truth of this observation. If the introduction of new names
more analogous to the object appears painful to the memory,
which was charged with another name for the same substance,
these changes will not fail at least to be very useful to those who
follow us in the mineralogical career. They will not be obliged,
like us, to charge their memory with a series of insignificant
names.

We ought not to be always occupied with ourselves, we ought
to think of posterity. It is sometimes as pernicious to follow
blindly those who have preceded us in any science, as it is hurtful
to wish to make every thing over again without distinguishing the
good from the bad. That we have not ourselves done a thing is
no sufficient reason for altering it. Those who will hereafter
reap advantage from our labours will find names much more
easily remembered, which indicate some striking property, geo-
metrical, chemical, or physical, or which recall to our recollec-
tion the name of some person to whom science is under great
obligations.

I will now finish this explanation of my ideas respecting the classification and nomenclature of minerals. I hope it will not be entirely unworthy of your attention, as it is the result of experience. I entreat you, Gentlemen, to consider this memoir, or rather this dissertation, as an introduction to several memoirs which I will take the liberty of presenting to you. My travels and my collection furnish observations which may be interesting to the mineralogist and the geognost. Allow me to recall to your recollection a well-known maxim, the truth of which is daily confirmed by your example, *Modesty is the signet of great talents.* Werner is the only person who gives the name of *arktizite* to a substance to which, out of gratitude, the name of *Wernerite* has been given. Haüy is the only person who has preserved the name of *latiolite* to the substance to which I gave the name of *Hauyne*, to recall to posterity the name of a man to whom the sciences have so many obligations.

ARTICLE IX.

On the Arctia Phæorrhæa. By Thomas Thomson, M.D. F.R.S.

In my walks in the neighbourhood of Chelsea this summer I was much struck with the appearance of the hawthorn hedges. A considerable portion of them was quite withered, as if they had been exposed to a sudden and intense frost. These withered patches sometimes extended for a hundred yards in length; then came a green portion, which was in its turn succeeded by another withered tract, and so on. The withered parts appeared quite covered with webs similar to that of the spider; and, upon a closer inspection, prodigious numbers of a black spotted caterpillar were seen enveloped in the webs, or hanging from them. During the month of May it was easy to find thousands of these animals devouring the leaves of the hawthorn. It was to their voracity that the withered state of the hedges was owing; for the whole leaves being destroyed, the plant ceased to grow, and assumed a wintery appearance. I observed this insect likewise upon elm-trees, and upon apple-trees; but not on beech, holly, yew, or privet, which were almost all the trees or shrubs that I observed in the hedges round about Chelsea.

The prodigious ravages committed by this insect induced me to make some inquiry respecting it; and though the facts which were stated to me have been long known to entomologists, yet I have thought them important enough to deserve notice in the *Annals of Philosophy*, as they may be of some utility to the farmer or gardener, and as they can scarcely fail to be an object of curiosity to every person who has noticed the ravages of this destructive insect.

The caterpillar is the larva of a white moth very well known in the neighbourhood of London, which usually makes its appearance in the month of June about sun-set, and deposits its eggs on the branches of those trees, the leaves of which afterwards serve the young caterpillar for food. It is usually called in England the *brown tail moth*, from the colour of its tail. Entomologists have given it the name of *arctia phæorrhæa*. It is the *bombyx chry-sorrhæa* of Hübner, and the *bombyx phæorrhæa* of Haworth. Neither Linnæus nor Fabricius seem to notice it, though it is the commonest of all insects in the neighbourhood of London and Paris. I do not recollect observing it on the hedges in Scotland; and certainly it never appears there in such abundance as to destroy the verdure of great portions of the hedges, as is the case in the neighbourhood of London.

During the years 1781 and 1782 these caterpillars appeared in such vast quantities in the neighbourhood of London, as to produce a considerable alarm. Daily paragraphs appeared in the newspapers respecting them, many of them of the most alarming nature. It was said to be the forerunner of the plague; it was represented as so abundant that it would devour all the grass of the fields, and starve the cattle; it was alleged that such multitudes of insects could not fail to infect the air, and produce a pestilential disorder; prayers were offered up in several of the churches for the removal of such a calamity. In some places they were collected by the common people at the rate of one shilling a bushel; and on the first day of this attempt no fewer than 80 bushels were collected in the parish of Clapham alone.

In the year 1782 Mr. Curtis published a small pamphlet on this insect, in order to quiet the minds of the people. It is to this pamphlet that I am indebted for the account of the insect which follows :—

These caterpillars are found on the hawthorn, oak, elm, most fruit-trees, blackthorn, rose-trees, bramble, and sometimes on the willow and poplar. They have never been observed on the elder, walnut, ash, fir, or herbaceous plants. Hence the only mischief they occasion is to rob particular trees of their foliage and blossoms. The trees and shrubs are not killed thereby; but, as soon as the caterpillars have removed to change to chrysalis, they put forth fresh foliage. The only loss, therefore, which the owner sustains, is some check to their growth, and a temporary deprivation of the beauties of spring and autumn. The case, indeed, is different with respect to fruit-trees, as by the destruction of the blossom or the bud the fruit for the season is destroyed. The owners of orchards and standard fruit-trees have, therefore, most reason to be alarmed.

Insects in general make their appearance in greater numbers after a hard winter than a mild one. The present summer is considered by the collectors of insects in the neighbourhood of London as the best that has occurred for many years. Accordingly various insects have been observed during it that had not made their

appearance in this neighbourhood for a series of years. Now it is needless to observe that last winter was the most severe since 1795. A mild winter is generally wet, and rain seems to be more injurious to insects than cold. Besides, a few days of warm weather brings them out of their lurking places, and then they are apt to be destroyed by the first severe day to which they are exposed.

The *brown tail moth* is of a white colour throughout, except a streak of brown on the under side of each fore wing, running near to and parallel with its anterior edge, and a brown, or mouse-coloured, tail, from which it derives its name. These moths come out of the chrysalis about the beginning of July, at which time they may be found flying about slowly, especially in the evening, and depositing their eggs on the foliage of the trees and shrubs which are adapted for the nourishment of the young caterpillars. The female has a much larger tuft of down on its tail than the male, a great part of which is made use of for covering its eggs, which when laid look like small lumps of down on the leaves.

" The young caterpillars are hatched early in autumn. As soon as they quit the egg they set about spinning a web ; and having formed a small one, they proceed to feed on the foliage, by eating the upper surface and fleshy part of the leaf, leaving the under side and the ribs. It is curious to observe with what regularity they marshal themselves for this purpose. Thus they proceed daily, spinning and enlarging their web, to which they retreat every night, and in bad weather, and extending their depredations. In the course of a few weeks their operations begin to be visible on the trees. Their web as yet is not so conspicuous, as those leaves which, being stripped of their green part, assume a dead appearance. Now is the time to destroy them, while their nest is small, and their ravages just conspicuous. They may be cut off the twigs or branches with a pruning knife or gardener's shears, whose handles may, if necessary, be lengthened ; or by a sharp hook affixed to the end of a long pole. When cut off, they should be collected together and burnt, merely to prevent their returning again to the trees and shrubs. By performing this operation thus early, you save the autumnal verdure of your foliage. If it be deferred till winter, the web will then be more conspicuous, and will have acquired a stronger and tougher texture, so as to bear pulling off, which should be preferred to pruning in certain cases, especially where it regards fruit-trees. No remedy short of removing the webs will avail. Lotions, fumigations, vermin powder, &c. will be applied to no purpose. The insects are too strongly enveloped to be affected by any of these. In about three weeks from their being first hatched, they change their skin ; a process which not only all caterpillars undergo four or five times at different periods of their growth, but also the spider, the bed bug, and even lobsters and crabs. This usually takes up several days. Afterwards they proceed in the same manner, enlarging their web, and extending their daily foraging excursions, till benumbing winter confines them

entirely to their silken habitations. They then not only secure the
general web to exclude impertinent intruders, but each individual
spins a thin case for itself. Here they rest in a state of torpid secu-
rity, till the genial warmth of spring animates them afresh, and
informs them that the all-bountiful author of nature hath provided
food convenient for them. Thus apprized, they issue forth in the
day time, and in fine weather, as before; but having acquired
stronger powers, and the foliage they have now to encounter being
more tender, they become less scrupulous in their feeding, and
devour the whole of it. A disposition to associate continues with
them till they have changed their last skins, when they usually
separate, each endeavouring to provide in the best manner for itself.
At this period they are most exposed to various enemies, and most
frequently attacked by the ichneumon fly. We sometimes find a
few continuing together to the last, when each spins a separate web,
in which it changes to a chrysalis. This usually takes place about
the beginning of June. Here, in a state of perfect quietude, it
remains for three weeks, when it changes to the moth already
described."

Article X.

On the Solubility of *White Oxide* of *Arsenic* in *Water.* By M. Klaproth.[*]

The solubility of white oxide of arsenic in water is a property
which essentially characterizes it. Though the fact has been long
known, yet the degree of solubility has not been accurately deter-
mined. According to Bergman, 80 parts of water at the tempera-
ture of 60° dissolve one part of white oxide of arsenic, while the
same quantity of oxide is dissolved by 15 parts of boiling water;
according to Navier, 80 parts of boiling water are requisite to dis-
solve one of the oxide; and according to Hagen, 30 grains of white
oxide of arsenic require four ounces of boiling water to dissolve
them.

These different statements induced me to endeavour to ascertain
the proportion; and the result of my experiments is, that three
parts of white oxide of arsenic may be kept in solution by 100 parts
of water at a medium temperature. On that account, the statement
of Aschof,[†] that one part of white arsenic requires 200 parts of
boiling water to dissolve it, appeared to me extraordinary.

As this result may occasion mistakes in medical jurisprudence, I
consider it as proper to point out the error of M. Aschof, and to
show that the new experiments which I have repeated on this sub-
ject have confirmed my former opinion.

A.—To determine the solubility of the oxide in cold water, I introduced 20 grains of it, previously reduced to a fine powder, into a flask containing 10 ounces of water of the temperature of 60°. This mixture was left for 24 hours, being often agitated in the mean time. The undissolved portion, collected upon a filter, and well dried, was eight grains. Of consequence, 12 grains had been dissolved. The result of this experiment is, that 1000 parts of cold water dissolve only $2\frac{1}{2}$ parts of white oxide of arsenic.

B.—Water can only saturate itself with this oxide at the boiling temperature. To ascertain the degree of solubility, I boiled for a quarter of an hour 200 grains of white oxide of arsenic in powder in four ounces of water in a phial. As soon as the undissolved part was deposited, I decanted off the liquid portion, which weighed 1800 grains. This liquid, evaporated in a capsule, the weight of which had been determined beforehand, left for residue 140 grains of white oxide of arsenic. Therefore 1000 parts of boiling water take up $77\frac{3}{4}$ of white arsenic.

C.—It was particularly important to know how much white oxide of arsenic the boiling water would retain after it was cold. For this purpose 10 ounces of boiling water were saturated with white oxide of arsenic in powder. After cooling, I left the phial for three days in cold water, during which time some white arsenic separated in the crystalline form. Five ounces of the decanted solution were evaporated in a capsule previously weighed. The residue, when well dried, weighed 72 grains, and was white oxide of arsenic. Hence it follows that 1000 parts of water retain in solution, after cooling, 30 parts of white oxide of arsenic, or 100 parts of water retain three parts of the oxide. It is obvious that the cold of winter may produce some modification in these proportions.

D. The crystalline form of the white oxide of arsenic obtained by evaporation may lead to the suspicion of the presence of water, or that the oxide is in the state of an hydrate, which might account for the augmentation of its weight. To determine this point, I boiled three ounces of water in a phial with 100 grains of arsenic. After a quarter of an hour's boiling, all the arsenic was dissolved. The clear solution, being evaporated to dryness, left 100 grains of white oxide of arsenic in a crystalline form. This experiment shows that oxide of arsenic does not combine with water when dissolved in that liquid, and evaporated to dryness.

ARTICLE XI.

On a Bed of fossil Shells on the Banks of the Forth.
By John Fleming, D.D. F.R.S.E.

THE bed of shells which I am about to describe occurs to the westward of the town of Borrowstounness in the county of Linlith-

gow, and stretches along the banks of the frith of Forth into Stir-
lingshire.

The shells which are found in this bed are all of them inhabi-
tants of the sea, and still exist in a living state. The common
oyster occurs in the greatest abundance, and occupies more than
three fourths of the bed. The common muscle may also be ob-
served, but in small quantity. The following shells occur more
sparingly : *patella vulgaris, buccinum undatum* and *lapillus, turbo
littoreus, nerita littoralis,* and *Venus pullastra.* There is little sand
or clay mixed with these shells, so that the bed is very open in its
structure. In the cavities formed by the shells resting upon one
another I observed the remains of several land shells. But a very
superficial examination convinced me that these were to be con-
sidered as foreign to the bed. In search of nourishment and shelter,
many specimens of *helix lucida, rufiscens, nemoralis* and *radiata,*
were, at the time I examined the bed, crawling among the marine
shells, and dead specimens of these were very common in the
cavities.

Many of the shells contained in this bed are of a very large size,
and must have belonged to very aged individuals. Among the
bivalved shells, there are a few perfect specimens, having the valves
still in contact. But in general the valves are detached, and the
shells broken into moderately sized fragments, so that the bed pre-
sents an appearance of confusion. The shells themselves are a little
altered in their texture. They are soft and friable, occasioned by
the decomposition of the animal portion of their constitution. The
epidermis is completely destroyed, together with the ligaments.

This bed appears to vary in thickness. Towards its eastern
extremity, where it is distinctly seen, it is upwards of three feet.
It there rests on a bed of small gravel, and is covered with clay and
soil. It preserves in all the parts of its course nearly the same
elevation above the surface of the river. In one place I ascertained
its height to be 33 feet above high water mark.

The extent of this bed is very remarkable. It makes its appear-
ance about two miles to the westward of the town of Borrowstoun-
ness, at a place called Craigenbuck. Here a limestone quarry has
been opened, and the excavation in the face of the bank has
exposed to view a very distinct section of the bed. This appears to
be its eastern extremity. It extends in a westerly direction along
the high bank on the south side of the road, and may be distinctly
seen at Inveravon. It has also been observed in the neighbourhood
of Little therse, and to the westward of the village of Polmont, in
Stirlingshire. It thus extends in a line parallel with the banks of
the Forth upwards of three miles. Between this high bank, in
which the bed of shells occurs, and the shore of the Forth, there is
an extensive deposition of alluvial soil called carse ground, nearly a
mile in breadth, and at present in a high state of cultivation.

The shells contained in this bed are common in the frith in a live
state, but at present we are not acquainted with any beds of oysters

or muscles of any extent so far up the river, nor nearer than eight
or ten miles. By what cause then, and at what time, have these
shells been deposited in their present situation?

When I first examined this bed of fossil shells, in 1806, I was
disposed to draw from it a proof of the gradual diminution of the
waters of the ocean, and the retreat of the sea from the British
shores. Several years afterward I read a short paper to the Wer-
nerian Society on the subject, which has been quoted by Professor
Jameson in his notes to Von Buch's Travels through Norway, page
218, as connecting the geological phenomena of Scotland with
those of Norway. In the latter country Von Buch observed at
various places beds of sea shells at different degrees of elevation
above the sea. At Tromsöe he found them at 20 feet, at Luroe
nearly 40 feet, at Hundolm 30 feet, and at Iteenkiär between 400
and 500 feet above the surface of the bay. In the last mentioned
situation the shells occur in a blue marly clay, and must have been
deposited at the same period with the clay. In the other places
where these shells have been observed, they are found much broken
and comminuted. In several places of Scotland appearances
similar to those observed in Norway have been traced.

At Paisley sea shells are found imbedded in a stratum of sand and
clay nearly forty feet above the present level of the Clyde. This
bed has been described by Captain Laskey. (*Annals of Philosophy,*
vol. iii. p. 150.) The ground on which the Botanic Garden of
Edinburgh is situated, after a thin covering of soil is removed,
consists entirely of sea sand very regularly stratified, with layers of
a black carbonaceous matter, in thin lamellæ, interposed between
them. The height of this ground is about 40 feet above the pre-
sent level of the sea, yet in this sand fragments of sea shells have
at different times been found. These facts certainly prove either
a sinking of the level of the ocean, or an elevation of the land;
but the bed which I have described above appears to have been
deposited during a violent and temporary agitation of the sea, and
in all probability does not belong to that series of geological pheno-
mena with which Von Buch and others have made us acquainted.

The shells in this bed are in many cases broken; but the frag-
ments are angular, and present no marks of continued attrition. In
this respect they prove themselves to have been deposited in a hurry,
and not to have been transposed from any distant quarter.

Almost all the shells are large and thick, and must have belonged
to aged individuals. In examining the exuviæ of the testaceous
mollusca thrown up by the sea during ordinary storms, we find
young and old shells blended together, but, in general, with a
greater proportion of the latter than of the former. In this bed
few young shells appear. Torn first from the rocks, they have pro-
bably been reduced to sand, while their aged sires have been raised
by violence from their beds, and shortly after have been thrown up
in the state of confusion and disorder in which they are at present
to be observed.

4

Farther up the Forth towards Stirling the same bed of shells occurs at a lower level, and much mixed with sand. The shells are broken into smaller fragments, and the bivalved species are seldom found entire. In Clackmananshire these shells rest on the out-goings of the floetz strata, of sandstone and slate clay, as stated by Mr. Bald, Mem. Wern. Soc. vol. i. p. 484.

From these facts I am disposed to conclude that this bed of fossil marine shells has been thrown up during some violent agitation of the sea, when the waves rose at least 33 feet above their ordinary limits.

This conclusion receives support from the appearances presented by beds of marine shells which have been thrown up by the sea during violent tempests. Thus on the shore to the westward of St. Andrews there is a bed of shells a little elevated above high water mark, and covered with the blowing sand which forms the *links*. This bed is nearly a mile in length, and about two feet in thickness, and presents the same general appearance of confusion which we have noticed in the bed of shells of the Forth. It is almost entirely composed of broken and detached valves of the lutraria vulgaris irregularly huddled together. During gales of wind the sea throws up at present a few of these shells, and in the time of a violent tempest (or what is termed by the fishermen a *grund-storm*) considerable quantities are cast up. These shells are found furrowed deep in the sand beyond low water. During the gale which brought these shells ashore the bed of sand must have been removed by the tide, and then the shells themselves transported to the shore. But to effect this a tempest infinitely more violent than any of those which have occurred for many years past must have happened. At present I can procure no information concerning its date.

Near Odness, in Stronsa, Orkney, there is another bed of marine shells, which has been deposited by the sea during a tempest. It is described by Mr. Neill in his Tour through Orkney, p. 27. This bed is elevated but a few feet above the high water mark. It is entirely composed of detached valves of the pectunculus pilosus and pecten maximus, of a large size. Similar beds are found in the island of Sanda. These shells are not littoral, like those which constitute the beds at the Forth and at St. Andrews. They inhabit deep water, and must have been forced to the shore by a very violent agitation of the sea. The natives of the island still remem-ber this dreadful tempest, which occurred about 30 years ago, and which threw up in the course of a single night several hundred cart-loads of these pelagic species.

We have very few accounts left us of great inundations of the sea which have taken place upon our coasts. Boece, in his Historia Scotorum, book iii. gives us a short description of the effects pro-duced by a violent tempest which took place about the year 1266. " Anno regni Alexandri tertii septimo et decimo, tanta inundatio, nimio plus solito maris estu per tempestates alveos excedente, facta est, prosertim Tai et Fortheæ fluviorum, ut multas villas ac pagos

prostraverit, maximamque cladem cum hominum tum pecorum dederit. Fordun supplies another fact in the history of this tempest, by stating that it came on accompanied with a violent north wind, and that its ravages extended from the Tay to the Tweed.

Such a storm must have left some visible traces of its existence. Tradition, indeed, mentions one of the effects of this mighty flood, in the destruction of a town, and in the elevation of the sands of Barrie, at the mouth of the Tay. And what prevents us from concluding that the same mighty tempest raised from the bottom of the channel of the Frith of Forth the oyster and muscles, and deposited them in a regular bed along the banks of the river?

Manse of Flisk, Fifeshire,
    June 17, 1814.

ARTICLE XII.

Astronomical and Magnetical Observations at Hackney Wick.
By Col. Beaufoy.

Latitude, 51° 32' 40·3" North. Longitude West in Time 6"$\frac{8\,8}{1\,0\,0}$.

Magnetical Observations.

1814.

Month.	Morning Observ.				Noon Observ.				Evening Observ.			
	Hour.		Variation.		Hour.		Variation.		Hour.		Variation.	
June 18	8ʰ	45'	24° 14'	43"	—ʰ	—'	—°	—'	—"	—ʰ	—'	—° —'	—"
Ditto 19	8	45	— —	—	1	05	24	24	04	7	35	24 16	27
Ditto 20	8	45	24 14	26	—	—	—	—	—	—	—	— —	—
Ditto 21	8	45	24 14	07	1	35	24	23	14	6	35	24 16	44
Ditto 22	8	40	24 11	43	1	35	24	24	25	7	05	24 16	27
Ditto 23	8	50	24 13	26	2	00	24	17	32	6	40	24 13	36
Ditto 24	8	45	24 12	42	1	15	24	21	16	6	35	24 18	47
Ditto 25	8	45	24 15	09	—	—	—	—	—	6	55	24 14	31
Ditto 26	8	45	24 11	22	1	30	24	21	52	—	—	— —	—
Ditto 27	8	45	24 16	27	1	45	24	22	47	6	40	24 18	16
Ditto 28	8	45	24 12	01	1	45	24	21	49	6	40	24 15	58
Ditto 29	8	40	24 10	54	1	30	24	22	43	—	—	— —	—
Ditto 30	8	45	24 13	36	1	55	24	22	51	—	—	— —	—

1814.

Mean of Observations in June. — Morning at 8ʰ 44'..... Variation 24° 13' 10" ⎫
Noon at 1 30 ..... Ditto 24 22 48 ⎬ West.
Evening at 6 52 ..... Ditto 24 16 18 ⎭

Ditto in May. — Morning at 8 45 ..... Ditto 24 12 49 ⎫
Noon at 1 44 ..... Ditto 24 22 13 ⎬ West.
Evening at 6 38 ..... Ditto 24 16 14 ⎭

Ditto in April. — Morning at 8 45 ..... Ditto 24 12 53 ⎫
Noon at 1 48 ..... Ditto 24 23 53 ⎬ West.
Evening at 6 29 ..... Ditto 24 15 30 ⎭

Ditto in March	Morning	at 8	52	..... Ditto	24	14	29	} West.
	Noon	at 1	52	..... Ditto	24	23	08	
	Evening	at 6	11	..... Ditto	24	15	33	
Ditto in Feb.	Morning	at 8	47	..... Ditto	24	14	50	} West.
	Noon	at 1	52	..... Ditto	24	20	58	
	Evening	at —	—	..... Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at 8	52	..... Ditto	24	15	05	} West.
	Noon	at 1	53	..... Ditto	24	19	03	
	Evening	at —	—	..... Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at 8	53	..... Ditto	24	17	21	} West.
	Noon	at 1	53	..... Ditto	24	19	49	
	Evening	at —	—	..... Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at 8	42	..... Ditto	24	17	42	} West.
	Noon	at 1	54	..... Ditto	24	20	24	
	Evening	at —	—	..... Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at 8	45	..... Ditto	24	15	41	} West.
	Noon	at 1	59	..... Ditto	24	22	53	
	Evening	at —	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at 8	53	..... Ditto	24	15	46	} West.
	Noon	at 2	02	..... Ditto	24	22	32	
	Evening	at 6	03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at 8	44	..... Ditto	24	15	58	} West.
	Noon	at 2	02	..... Ditto	24	23	32	
	Evening	at 7	05	..... Ditto	24	16	08	
Ditto in July	Morning	at 8	37	..... Ditto	24	14	32	} West.
	Noon	at 1	50	..... Ditto	24	23	04	
	Evening	at 7	08	..... Ditto	24	13	56	
Ditto in June	Morning	at 8	30	..... Ditto	24	12	35	} West.
	Noon	at 1	33	..... Ditto	24	22	17	
	Evening	at 7	04	..... Ditto	24	16	04	
Ditto in May	Morning	at 8	22	..... Ditto	24	12	02	} West.
	Noon	at 1	37	..... Ditto	24	20	54	
	Evening	at 6	14	..... Ditto	24	13	47	
Ditto in April	Morning	at 8	31	..... Ditto	24	09	18	} West.
	Noon	at 0	59	..... Ditto	24	21	12	
	Evening	at 5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
July 1	8h 45'	24° 12'	07"	1h 55'	24° 22'	00"	7h 15'	24° 17'	42"
Ditto 2	8 45	24 13	14	1 35	24 23	05	6 50	24 17	10
Ditto 3	8 45	24 13	31	1 30	24 24	09	7 25	24 17	31
Ditto 4	8 45	24 12	15	1 03	24 23	55	6 50	24 17	30
Ditto 5	8 50	24 13	38	1 55	24 26	45	6 55	24 19	09
Ditto 6	8 45	24 15	25	1 45	24 22	43	7 15	24 17	05
Ditto 7	8 50	24 13	21	1 45	24 21	20	6 45	24 16	38
Ditto 9	8 35	24 14	06	1 35	24 24	25	6 55	24 18	27
Ditto 11	8 35	24 13	05	1 30	24 25	04	6 55	24 16	26
Ditto 12	8 55	24 14	19	1 50	24 25	47	6 35	24 17	06
Ditto 13	8 50	24 15	09	1 40	24 23	19	6 45	24 17	30
Ditto 14	8 40	24 13	36	1 40	24 23	26	6 50	24 16	00
Ditto 15	8 40	24 12	41	1 35	24 24	01	6 50	24 16	21
Ditto 16	—	—	—	—	—	—	6 50	24 17	17
Ditto 17	8 35	24 13	38	1 30	24 24	50	6 50	24 18	10

July 7.—The needles vibrated 8' 30" : and the next day proved very rainy.

July 9.—The needles vibrated 6′ 30″ ; and the following day was also very rainy.

Rain fallen { Between noon of the 1st June } 2·041 inches.
            { Between noon of the 1st July }

Article XIII.

On the Extrication of Caloric during the Coagulation of the Blood. By John Gordon, M.D. F.R.S.E. and Lecturer on Anatomy and Physiology, Edinburgh.

(To Dr. Thomson.)

DEAR SIR,   Edinburgh, July 1, 1814.

My friend Dr. John Davy, in his interesting thesis (Quædam de Sanguine Complectens) printed here last month, has alluded to an experiment of mine, relating to the extrication of caloric during the coagulation of the blood, which I have been accustomed to mention in my anatomical and physiological lectures for three years back. In the lectures on physiology which I delivered during the three summer months of last year, the limited period of the course did not permit me to dwell so long on that, nor on many other subjects, as I could have wished. To this, I doubt not, it is to be attributed, that the experiment referred to has been in some degree misapprehended by Dr. Davy ; and if by him, who honoured me with the most flattering attention during the whole of that course, I fear also by many other of my pupils.

The following, however, are the notes relating to this subject, from which I then read :—

" As it is a fact established in chemistry that the conversion of a fluid into a solid is always accompanied with an extrication of caloric, one could have little doubt, even if the fact were not made evident by experiment, that caloric is extricated during the coagulation of the blood.

" Fourcroy had stated it, as the result of experiments made by him at the Paris Lyceum, in 1790, that during the coagulation of bullock's blood as much caloric was given out as raised the thermometer (Reaumur's, I presume,) five degrees. (Ann. de Chimie, t. vii. p. 147.)

" But more authority seems to have been attached by physiologists to the following experiment of Mr. John Hunter. Mr. Hunter having suspended a healthy turtle by the hind legs, cut off its head, and caught the blood in a bason. The blood while flowing was 65°, and when collected was 66°, but fell to 65° while coagulating, which it did very slowly. It remained at 65°, and when coagulated was still 65°. From this, and similar experiments, Mr.

Hunter concluded that in the coagulation of blood no heat was given out. (Treatise on the Blood, &c. 4to. p. 27.)

" A very different result, however, has since been obtained by the author of a short article on the blood in Rees's Cyclopædia. Ten ounces of blood were drawn into a wooden bowl, in which a thermometer was held. The temperature of the blood while flowing from the vein was 93°. In six minutes the thermometer had sunk to 89°, and coagulation commenced on the surface. On elevating the bulb of the thermometer to the coagulum on the surface, the mercury rose to $90\frac{1}{2}°$, and on again depressing it to the bottom of the bowl it sunk to 89°. This was repeated twice, with nearly the same result ; and on the third trial the quicksilver rose to 91°. The blood was now coagulated throughout ; and after this, the mercury continued to descend regularly, and was no longer influenced by changing the situation of the bulb of the thermometer. In this experiment it clearly appeared that during the coagulation of the blood caloric was extricated ; and in sufficient quantity, at one time, to raise the thermometer 2°.

" As it was desirable to confirm this result, and, of course, that before obtained by Fourcroy, both so conformable to analogy, my friend Mr. Ellis and myself, in presence of Professor Thomson, performed the following experiment, in the month of April, 1810:

" Blood was received from the femoral artery of a dog, into a small glass jar. The temperature of the blood flowing from the artery was 99° Fahr.; and that of the apartment, during the experiment, 46° Fahr.

" One minute after the blood had been received into the vessel, it began to coagulate, by a film on the surface. The bulb of a very delicate centigrade thermometer was now placed into the blood at the upper part of the vessel, and held there during a minute, without touching the sides of the glass. It was then depressed to the lower part of the vessel, where coagulation had not begun, and held there in the same manner during the next minute. During the next it was held at the top, and during the next at the bottom ; and so on, it was alternately elevated and depressed for 20 successive minutes after coagulation had begun on the surface.

" When the bulb was first placed in the blood at the top, the mercury gradually rose to 34° ; but when it was depressed towards the bottom, it instantly fell to $30\frac{1}{2}°$. When again elevated, it rose to $33\frac{1}{2}°$; and when again depressed, sunk to 30°. A third time brought to the surface, the mercury rose to 32° ; and a third time depressed, it fell, in half a minute, to $28\frac{8}{10}°$ At the next elevation the mercury rose to 31° ; and at the succeeding depression, fell to $28\frac{1}{2}°$. At $18\frac{1}{2}$ minutes after the blood had been drawn, when the bulb of the thermometer was brought from the bottom towards the top, the mercury rose from 24° to $25\frac{1}{2}°$. It was now held at the top for two minutes, and the mercury gradually fell to 24°. The blood seemed now completely coagulated, and the experiment was discontinued.

" In this experiment, therefore, the extrication of caloric during the coagulation of the blood was rendered sensible by the thermometer for 20 minutes after the process had commenced ; and was at one period so great as to raise the thermometer, in this cool apartment, $3\frac{1}{2}°$ of the centigrade scale, which is equal to 6·3° Fahr.

" That a similar extrication of heat was not apparent in Mr. Hunter's experiment on the blood of the turtle, may have been owing to his not having placed the bulb of the thermometer alternately in the coagulating and yet uncoagulated part ; and partly, perhaps, to the slowness with which he informs us the process went on.

" We may regard it, therefore, as established, that when part of the blood thus spontaneously passes from a fluid to a solid state, caloric is extricated, in the same manner as when other fluids undergo a similar change."

Farther than this, my notes did not then extend.

But during last winter I had an opportunity of trying this experiment several times, on venous blood drawn from persons labouring under inflammatory complaints, and the result was always precisely similar. The following is a note of one of these experiments :—

Jan. 22, 1814. I received three ounces of blood from the median vein of a man, aged 40, labouring under pneumonia, into a tall glass vessel, and immediately introduced into it a delicate Fahrenheit thermometer, the bulb touching the bottom. The temperature of the blood in this situation was 76°. In two minutes fluid *size* collected at top ; and in two more a very thin film appeared on the surface of this size, and the thermometer was then exactly 74°. In four minutes more, that is, eight minutes after the blood had been drawn, a soft coagulum was formed to the depth of an inch from the top, and the thermometer (the bulb being still at the bottom) was 73°. I now raised the bulb smartly into the middle of this coagulum, and instantly the mercury rose to 85° (that is, 12°); and when I depressed it again to the bottom, where the blood was still fluid, the mercury immediately sunk to 73°. I repeated this several times with similar success. The temperature of the apartment during the experiment was 55°.

I have tried this experiment again, within these few days, on blood drawn from the arm of a middle aged man, labouring under an affection of the heart, and the result was similar.

On carefully reviewing all these experiments, I cannot discover any source of fallacy connected with them, which should lead me to hesitate in deducing from them the same conclusion as formerly, viz. that caloric is extricated during the coagulation of blood, and therefore that the blood is no exception to the general law applicable to all other fluids in this respect.

That my friend Dr. Davy has been led to adopt an opposite opinion in consequence of his experiments on lamb's blood, has obviously arisen from his not having been aware of the importance of moving the thermometers in his experiments ;—a circumstance

for a knowledge of which I am myself entirely indebted to the anonymous author already referred to in Rees's Cyclopædia; and a circumstance, I may add, which, unless it be scrupulously attended to, is calculated to render all experiments made with a view to ascertain the temperature of blood, after it has been drawn from the vessels, altogether inconclusive. At the same time, I cannot help observing, though with the utmost deference to one so much more familiar with chemical details than I am, that, taking Dr. Davy's experiments as they are, they rather seem to me to warrant an opposite conclusion to that which he has drawn from them; and that, upon the whole, they rather tend to confirm than to disprove the extrication of caloric during the coagulation of the blood.

I am, Sir, yours, &c.

JOHN GORDON.

ARTICLE XIV.

ANALYSES OF BOOKS.

An Account of the Basalts of Saxony, with Observations on the origin of Basalt in general. By J. F. Daubuisson, Member of the National Institute, and one of the principal engineers to the Board of Mines in France. Translated, with notes, by P. Neill, F.R.S.E. and F.L.S. Secretary to the Wernerian Society. With a Map of the Saxon Erzgebürge, from Petri. Edinburgh, Constable and Co. 1814; London, Longman and Co. One Vol. 8vo.

THIS book has been long well known to mineralogists. Mr. Neill has conferred a favour on the British mineralogical public by his translation of it. His notes are not numerous, but they are judicious, and relate chiefly to Scotland, a country in which basalt abounds, and which, in a mineralogical point of view, is very interesting. The map is convenient and useful, and gives to the present translation a superiority over the original work.

Three opinions respecting the origin of basalt, and the minerals that usually accompany it, as greenstone, and porphyry slate, have long divided the geological public. One party, at the head of which is Werner, consider this mineral to have had a similar origin with the other rocks of which the crust of the earth is composed, that is to say, to have been deposited from a liquid, which formerly covered the whole surface of the earth. Another party, headed by Voight and by Dolomieu, &c. conceive basalt to be lava, and all basaltic mountains to be the remains of extinct volcanoes. While a third party, headed by Dr. Hutton, of Edinburgh, and consisting chiefly of Sir James Hall and Mr. Playfair, with two or three young proselytes, maintain that basalt was fused by the central fire of the

earth while at the bottom of the sea, and that it was afterwards
raised up to its present height by some unknown but powerful
natural agent, to which all the present mountains on the earth's
surface owe their elevation. It was in order to determine, by an
appeal to facts, which of these opinions is supported by the
strongest evidence, that Daubuisson was induced to examine the
basalt of Saxony, and to compose the present work.

Most of our readers, we presume, are acquainted with the stone
to which the name basalt is applied by mineralogists. It has a
greyish black colour ; and when polished, a bluish aspect. Some-
times it has a slight shade of green or brown. It occurs in beds,
and usually occupies the summit of mountains. Its fracture is dull,
and almost earthy, but fine-grained. It often presents distinct
granular concretions. It is difficultly frangible. It has a tendency
to form six-sided irregular prisms, some very striking examples of
which may be seen in the county of Fife. Its hardness varies.
Sometimes it strikes fire with steel, in which case it has a con-
choidal fracture ; sometimes it may be scratched with a knife. Its
specific gravity is about 3·065. It usually affects the magnetic
needle. When exposed to the weather it gradually crumbles down
into a fine black mould, which constitutes a very fertile soil. It is
to this rock that some of the richest parts of Scotland owe their
fertility. The only foreign substances usually observed in it are
crystals of basaltic hornblende, mica, felspar, augite, and olivine.
Sometimes it contains vesicles filled with green earth, chalcedony,
calcareous spar. In its composition it agrees nearly with green-
stone, a well known rock composed of felspar and hornblende, with
which it alternates, or into which it passes ; and there is reason to
believe that both are composed of the same materials mixed in a
confused manner in the basalt, but symmetrically arranged and
crystallized in the greenstone. Fine specimens of basalt may be
picked up on the road near Edinburgh, especially towards Roslin ;
for in that quarter rocks of basalt are often employed as materials
for mending the road. They look exceedingly beautiful after a
shower of rain, and can scarcely avoid attracting the eye of the
mineralogist.

Daubuisson divides his work into five parts. In the first he gives
definitions of the terms which he employs, and explains the nature
of the stones which occur in the basaltic mountains. In the second
he gives a description of the basaltic mountains of Saxony, such of
them, at least, as he examined particularly. In the third he draws
conclusions respecting the origin of the basalt in these mountains,
founded on his preceding observations. In the fourth he produces
proofs to show that the Saxon basalt cannot be of volcanic origin.
And in the fifth he gives us his inferences respecting basalt in
general. But these inferences he was afterwards induced to modify,
by his examination of Auvergne, where the rock occurs in situations
that seem to leave no doubt about its volcanic origin.

The basaltic mountains in Saxony which our author describes lie

chiefly in the Erzgebürge, or metalliferous mountains, a chain of
mountains which separates Saxony from Bohemia. It extends in
length about 120 miles, terminating in one extremity in Franconia,
and in the other in the great and deep valley occupied by the Elbe.
Its height is about 3280 feet above the plains of Saxony, or 3600
above the level of the sea. The declivity towards Bohemia is very
rapid, but towards Saxony it is quite gradual. The fundamental
rock of this chain is granite; which is covered, or, as it were,
wrapped round, by beds of gneiss, mica slate, and clay slate, lying
above each other in the order in which they have been named. In
many places the granite seems to pierce the covering, and appear
visibly. Among these beds there are some which contain metallic
minerals. These, as well as the numerous and rich veins which
traverse them, are the objects of the great mining operations in
Saxony. There occur likewise, in the chain rocks of serpentine
and of quartz, beds of limestone, of coal, of clay, and others. The
whole of the eastern part of the chain is covered on the north side
with a huge bed of porphyry, and on the south side by a bed of
sandstone of equal magnitude.

It is upon these mountains that the Saxon basalt treated of by
Daubuisson rests. It forms the summit of about twenty mountains,
under various forms, as tables, cones, and domes. Some of these
mountains are isolated; but they are more generally connected by
their sides to the neighbouring mountains, the basaltic top alone
remaining separate. The chief basaltic mountains which he de-
scribes are the following :—Scheibenberg, Bærenstein, Pæhlberg,
Heidelberg, Ascher-hubel, Landberg, Steinkopf, Lichtewalde,
Geissengenberg, Luchauerberg, Cottanerspitze, Winterberg, Heu-
lenberg, and Stolpen.

Scheibenberg is a conical mountain composed of gneiss, which is
covered by beds of mica slate and clay slate. Situated in a kind of
plain formed by the highest part of the body of the mountain, we
find a bed of gravel, over which there is one of fine sand, and then
one of clay. On these horizontal beds of gravel, sand, and clay, lies
the mass of basalt which crowns the mountain. It is about 750
feet in length, 400 in breadth, and between 200 and 260 in thick-
ness. Its upper surface is nearly horizontal. On the west side
many subterraneous galleries have been opened, which have been
pushed under the basaltic platform. These operations have dis-
closed in part the beds on which the basalt rests. In 1787 M.
Werner observed in this place a bed of wacke on which the basalt
was deposited in immediate contact; and he remarked that these
two substances passed into each other by gradual shades of differ-
ence.

The basalt which constitutes the summit of Pæhlberg likewise
lies over beds of gravel, sand, and clay, deposited in the same
order as in Scheibenberg. In the mountain called Bærenstein the ba-
saltic platform likewise lies immediately over a thin bed of sand. In
the mountains of Landberg, Ascher-hubel, Cottanerspitze, and Heu-

lenberg, the basalt rests immediately upon sandstone; in Steinkopf, Geissingenberg, and Luchauerberg, it rests upon porphyry; in Spitzberg it rests upon mica slate; in Lichtewalde, Stolpen, and Landscrone, it seems to rest on granite; and in Heidelberg it rests upon gneiss.

From his examination of the structure of the basaltic mountains of Saxony, Daubuisson draws the following conclusions. The basalt every where forms the uppermost rock of these mountains. No traces of it coming from below can be observed. Hence it must have been formed from above, and of course must have been the last formed of all the rocks, and probably long after all the other rocks constituting these mountains had existed. Basalt possesses similar characters with other rocks, and therefore may have been formed in the same way with them. It is composed of the same constituents as greenstone, and gradually passes into greenstone on the one hand, and wacke on the other. He conceives the basalt to have once constituted a bed which covered the whole country, and to have been all wasted away except the few fragments which now cap the summits of some of the hills.

In the fourth part of his work Daubuisson refutes the opinions of those who consider the Saxon basalt as having flowed from a volcano. Two hypotheses on this subject present themselves. We may either suppose that each basaltic mountain has been a distinct volcano, or we may suppose that all the basalt of Saxony has flowed from one great volcano which now no longer exists. The author shows that these basaltic mountains can never have been volcanoes, because there is no analogy between them and volcanic mountains. They are regularly stratified, which is never the case with volcanic mountains; no traces of a crater can be perceived, nor any thing similar to the lava vomited out of volcanoes, unless the masses of basalt, the position of which is not such that it can have been in a state of fusion, be considered as such. We cannot, then, with any shadow of probability, allege that these basaltic mountains have been volcanoes. The notion that the basalt has flowed from a single crater now no longer in existence is still more absurd, if possible. No traces of ashes, scoriæ, cinders, can be any where observed, though these mountains have been every where explored for centuries, so that the interior is nearly as well known as the exterior. The basalt, if lava, must have been the result of one eruption which covered the whole surface of the mountains, to an extent of 600 square miles, with an even bed of basalt nowhere more than 800 feet in thickness. Such a supposition is obviously absurd, and quite inconsistent with the nature of lava, or with the common principles of hydrostatics, by which the motion and figure of lava must be regulated. From these considerations our author conceives the supposition that the Saxon basalt has proceeded from a volcano as altogether untenable.

Our author next considers the origin of basalt in general, and endeavours to show that it could not possibly have been produced

from volcanoes. His proofs are as follows :—I. *From the nature of basalt.* Specimens of basalt occur consisting at one end of perfect basalt, at the other of clay, and these two substances pass insensibly into each other. Such a mineral could not possibly have been ejected from a volcano. Greenstone and basalt are found intermixed, and gradually passing into each other, at the Meisner mountain, and in other places. Now as greenstone cannot have been acted on by fire, it follows that basalt likewise could not have been formed by any such agent. Basalt, from what place soever it comes, is always possessed of the same characters, and yields the same constituents. But this is not the case with lava, which varies considerably, according to the nature of the rock, by the melting of which it has been formed. Hence basalt must be an aqueous production, as these alone exhibit that uniformity of composition and properties for which basalt is so remarkable. Basalt contains about 20 per cent. of iron. Now there is no rock that could furnish it with such a proportion. Hence it cannot have been formed by the fusion of any other rock. II. *From the heterogeneous substances contained in basalt.* These substances are, 1. Crystals or grains of basaltic hornblende, olivine, augite, mica, and felspar. These are regularly distributed as in porphyries, which could not be the case if the basalt had been in a state of fusion. They retain their crystalline characters, and do not exhibit the slightest traces of the action of fire ; though some of them, as felspar, are more fusible than the basalt itself. They exactly fill the cavity in which they are deposited, whereas in lava the crystals are smaller than the cavities in which they are, and therefore often rattle when the lava is shaken. All these circumstances are inconsistent with the notion that basalt has been in a state of fusion, or that it is lava. 2. Cavities usually of a round shape, some empty, and some occupied wholly or in part with balls or geodes of green earth, steatite, calcareous spar, zeolite, chalcedony, quartz, and other substances. They differ from the grains already mentioned in being evidently of posterior formation to the basalt in which they are found. Now no such substances ever occur in true lava, which never has an amygdaloidal structure. 3. Fragments of older rocks. Fragments of sandstone and of limestone quite unaltered occur in basalt. Petrifactions also have been seen in it, though rarely. Such appearances are quite inconsistent with the notion of its being lava. 4. Basalt contains about five per cent. of water of composition, while no water whatever is found in lava. III. *From the form of basaltic rocks.* Basalt forms regular beds, frequently very thin, and extending for a very considerable way. It is inconsistent with the nature of lava to take any such form. Therefore basalt could never have been in a state of fusion. IV. *From the position of basalt with regard to other rocks.* It sometimes lies immediately over or under coal, as is the case in the Meisner mountain ; yet the coal is not the least altered in its nature, though some alteration must undoubtedly have been produced if the basalt,

at the time of its deposition, had been in a state of fusion from heat. Basalt sometimes alternates with limestone ; and Dolomieu describes no fewer than 20 beds of basalt alternating with as many beds of limestone containing marine shells. All these, and many other examples quoted by Daubuisson, are inconsistent with the notion that basalt is lava. The same incompatibility follows from the fact that the rock immediately under the basalt, whether clay, sand, mica slate, gneiss, granite, or porphyry, is not the least altered in its texture, as would undoubtedly have been the case had the basalt been deposited on it in a state of fusion from heat.

M. Daubuisson concludes his essay with a general review of the whole, in which he seems rather to incline to the opinion that basalt is always of aqueous origin, and never produced by heat. He finally gives a more detailed account of the properties of basalt than he had given at the commencement of his essay.

Of the notes that accompany this essay, the only one of which it seems requisite to give an account is that in which the result of Daubuisson's examination of Auvergne is given, because it informs us that he has found it necessary to modify somewhat his former opinions on this subject. He was advised by the French Institute, some of whom are zealous volcanists, to make this tour, and he performed it in 1804. The base of Auvergne he found to be granite. In the western part it is covered by gneiss and mica slate, containing metallic veins, and in some places with greenstone slate. In the valley of the Allier, limestone appears containing silicious concretions, and much impregnated with bitumen. In the low district, where the Alagnon falls into the Allier, a coal country occurs. Every where else the granite serves as the immediate support of the volcanic hills. The chain of *Puys* extends for above 20 miles. Most of them stand detached. Their form is generally that of a truncated cone ; their sides are inclined at an angle of about 30°, and on their summits there is generally a cup-like depression, which is sometimes 200 feet deep. Their general elevation is from 900 to 1300 feet above the plain ; the central and highest, the Puy de Dôme, being near 2000 feet.

The substances which chiefly compose these hills are described under the titles of Spongy Scoriæ, which are often of a bright red colour ; twisted, or rope-like scoriæ (*scories cordées*) ; lapillo, or volcanic gravel ; vesicular, or blistered lavas (*laves boursoufflées*) ; and basaltic lava (*lave basaltique*). In most cases these substances are confusedly heaped together ; but the Puy de Dôme, and two or three others, form exceptions.

The sides of most of the Puys are covered with herbage and brushwood ; a few are wooded. On the top, as already mentioned, is generally found a crater, which is sometimes broken down on the side from which the lava had flowed. One of the most perfect examples is the Puy de Pariou ; and Mr. Daubuisson's description of this is given at full length in the Journal de Physique. Its crater is so entire, and so nicely formed, that it appears as if "turned on a loom ;"

and the stream of lava, 200 feet broad, is observed, after having flowed for some way, to divide into two, the smaller currents then making their way to the low grounds in different directions, to the distance of four or five miles. These lava streams receive the general denomination of *cheires* from the inhabitants of the country. They commonly form a most barren and ungrateful soil : the cheire that proceeds from the Puy de Come is so remarkable for its sterility that it is called *cheire de l'aumone*.

The surface of the lava is generally blistered and extremely rough, some of the asperities projecting several feet. An idea of the roughness of its surface may be formed, by supposing it to resemble that of melted lead poured into water. The interior of the lava is of a greyish black colour, of a fine compact grain, hard, brittle, and frangible, differing from common basalt in these last characters. It contains crystals of augite and of felspar, the latter having a vitreous aspect, and rarely grains of olivine. The lava of the Puy de la Vache, however, very much resembles common basalt. It is vesicular at the surface, but fine-grained or compact in the interior or at the bottom.

The cheire of Volvic is in some respects an exception. Its substance is of a lighter colour, and so soft that it is easily worked. Quarries have therefore been opened in it, and it affords a pretty good building material. It contains pieces of quartz altered by the heat; and the walls of the cavities which occur in it are set with spangles of specular iron ore. This cheire of Volvic had evidently flowed from the Puy of Nugere. A spectator standing on the summit of this hill has a distinct view of the stream of lava which had issued from the crater beside him. He perceives that, in descending to the low ground, it had spread over a large basin, bounded by rocks of granite, and had surrounded some detached rocks which interrupted its progress. The tops of these detached granite rocks are seen rising above the surface of this lake of lava.

" When I thus perceived (says Mr. Daubuisson) that these lava streams had avoided obstacles, and obeyed all the laws of hydrodynamics, I could not help reflecting, that if the infringement of these laws by the basalts of Saxony had formerly influenced my opinion against admitting *their* igneous formation, the exact obeying of the same laws in this case ought to produce an opposite conviction in regard to *those of Auvergne*."

It appears therefore, as already mentioned, that if Mr. Daubuisson was formerly inclined, from analogy, to consider *all* basalts as of aqueous origin, he has in so far altered his views, that he now excepts *those of Auvergne*. This seems to be the extent of any change of sentiment ; and it is without reason that some incautious writers have stated, that Mr. Daubuisson has entirely altered his opinion concerning the origin of basalt in general. His opinion, stated in the most favourable way for the volcanists, may be supposed to amount to this, that a substance nearly identical in character and nature may have been produced either from fusion or

from solution.   He declares that some of the " *laves basaltiques,*"
or " *laves de nature basaltique,*" of Auvergne, bear the most
striking resemblance to the " *basalte ordinaire* " of Germany ; but
it may be remarked, that even the difference of nomenclature indi-
cates that he considered them as of different origin.

Mr. Daubuisson seems perfectly aware of the difficulty of ac-
counting for the production of lava in a granite country, and where
the volcanic hills rest immediately on the granite.   The granite
itself could not afford the lava ; for this contains from 15 to 20 per
cent, of iron ; while the granite contains scarcely any.   The rocks
which afforded the lava must therefore necessarily be situated below
the granite.   Yet the granite is, in some of the valleys, observed to
be more than 1200 feet thick.

Article XV.

Proceedings of Philosophical Societies.

royal society.

On Thursday the 30th of June, a paper by Sir Everard Home,
Bart. was read, On the Influence of the Nerves on the beating of
the Arteries.   He was led to his opinion of this influence, by the
case of an officer who had received a ball in the leg.   The ball was
lodged among the fractured parts of the tibia ; and after its extrac-
tion, an attempt was made to remove some parts by the applica-
tion of caustic alkali ; but the pain produced was so great that they
were obliged to desist.   The pain was not in the part to which the
alkali was applied, but at some distance, and seemed to result from
the violent beating of the arteries.   Hence it was ascribed to the
action of the alkali on a nerve, and the consequent reaction of this
nerve upon the arteries.   Upon laying bare the carotid artery of a
rabbit, and applying caustic alkali to the intercostal nerve, the
artery began to beat violently, and continued to do so for some
time.   This fact, in the author's opinion, throws considerable light
on the action of the arteries in various parts of the animal economy,
hitherto but imperfectly explained.

At the same meeting, a paper by Smithson Tennant, Esq., On
a Method of economizing Fuel during Distillation, was read.   Dr.
Black long ago demonstrated, that the quantity of heat requisite to
raise water from the common temperature to a boiling heat, is only
about $\frac{1}{6}$th of what is requisite to convert it into steam.   Hence if
steam be made to act on cold water, it speedily raises it to the
boiling point ; but as it cannot make it boil, water heated by steam
does not distil over in any considerable quantity.   Mr. Tennant's
improvement consists in this.   The worm of a common still is
made to pass as usual through a vessel containing water.   This

vessel is made air-tight, and is made in the shape of a still and receiver. As soon as the common still is made to boil, the steam is conveyed into the receiver by means of pipes, and allowed to pass till it has expelled the air ; then the stop cocks are shut, and the steam passes through the worm as usual. It speedily heats the water surrounding the worm, which in consequence of the vacuum distils over in considerable quantity.

At the same meeting, part of a paper by Mr. Porrett was read ; On the Salts commonly called triple *Prussiates.* Mr. Porrett began by stating clearly and concisely the striking differences between the common prussiates, made by uniting prussic acid directly to the bases, and the triple prussiates made by boiling the base upon prussian blue, The triple prussiates contain in all cases the black oxide of iron ; yet its presence cannot be detected by any reagent. The reason is, as Mr. Porrett has ascertained, that the triple prussiates are not in reality triple salts, nor do they contain any prussic acid. They consist of an acid hitherto unknown, combined with the base, and neutralized by it. This acid he calls *ferrureted chyazic* acid, (a name composed of the first letters of the words carbon, hydrogen, and azote, with the syllable *ic* added), because it is composed of black oxide of iron, carbon, hydrogen, and azote. When triple prussiate of soda is dissolved in water, and the solution acted upon by the galvanic battery, the soda appears at the negative extremity, while the oxide of iron and prussic acid are evolved at the positive extremity, and, uniting, constitute Prussian blue. Had not the iron constituted a part of the acid, had it been a base, it would have been evolved at the negative extremity of the battery.

Mr. Porrett dissolved a quantity of triple prussiate of barytes in water, and added to it a quantity of sulphuric acid exactly sufficient to separate all the barytes. The consequence was, that the sulphate of barytes, separating the acid of the triple prussiate, remained in solution in water. It had a yellow colour, and no smell. When slightly heated it was decomposed, the white prussiate of iron falling down, which speedily became blue by absorbing oxygen.

Other acids may be obtained by the combination of other substances besides oxide of iron with the compound base of prussic acid. Thus sulphur combines with it, and forms what the author calls sulphureted chyazic acid, which has the curious property of precipitating peroxide of iron blood red. I regret that the remainder of this curious paper was not read, so that it is not in my power to continue the account of it any further. At the end of the session of the Royal Society, there is always such an overflow of papers, that it is not possible to read them all quite through in one night. Hence they are generally curtailed.

A paper was also read, written by Mr. Houghson, on the formation of bones. For the reason just assigned, I can give but an imperfect account of it. From his observations, made on fœtuses, the author concludes, that the first commencement of bone is an

exudation on the perosteum, then the cartilage is formed in which the bony matter is gradually deposited. The deposition is reticulated, owing to the nature of the substance in which it takes place.

The Society adjourned to Thursday the 10th of November.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(*Continued from* p. 74.)

M. Mathieu, an astronomer attached to the Imperial Observatory, has presented to the Class two memoirs, one on the solstices, the other on the latitude of the Imperial Observatory. These two points had been discussed and established from a considerable series of observations, by the author of the tables of the sun, and of the base of the metrical system. The circles which Mechain and himself had employed in these observations, were those of Borda. It is known that the indefinite multiplication of angles offers to the patience of the observer a method of diminishing at pleasure the error of the result. But it was feared, that the small magnifying power of the telescope might have occasioned errors, which being nearly the same for each observation taken apart, would not offer the necessary compensation. It was thought necessary to possess an instrument of a greater radius, which would at least diminish the trouble of the observers, lead with less labour to the same precision, and furnish an important verification; or probably even add to the accuracy of the result. This wish of philosophers was fulfilled by M. le Comte Laplace, who having obtained a circle of Reichenbach, made a present of it to the Imperial Observatory. M. Arrago and M. Mathieu were charged with the verification of these points, the most delicate in astronomy. They have already made a long series of observations of every kind. M. Mathieu calculates them, and lays before the Institute the most remarkable results drawn from them.

The magnifying power of the telescope enabled them to distinguish the pole star during the day. With the circle of Borda it was scarcely visible in the twilight, and its diameter was smaller than that of the micrometer wire. When the wire covered it, there was no means of knowing if it was equally distant from its two edges. Hence the observation might easily be wrong a second. On that account, observations made during the night were preferred, because then the star touched the wire on both sides. With the telescope of Reichenbach it is quite the contrary. The wire is so fine, that at night the star greatly exceeds it in diameter. One is less sure of being able to divide it by the wire exactly into two equal parts. The excess is much smaller during the day, and hence observations made at that time are more to be depended on: notwithstanding this small disadvantage, the nocturnal observations agree as well as the others. The difference of all the observation

from the mean are, once $+ 2 \cdot 36''$, once $1 \cdot 15''$, and another time $- 2 \cdot 14''$. Among the others, the greatest differences do not amount to a second. Such is the case with the superior transit. With respect to the other transit the deviations are greater, namely, $+ 2 \cdot 28''$, $+ 2 \cdot 15''$, $+ 2 \cdot 11''$; $- 1 \cdot 80''$, $- 1 \cdot 22''$, the rest do not exceed a second. The mean result is

For the latitude of the circle............$48°$ $50'$ $13 \cdot 82''$

For the southern face of the observatory.....48 50 $13 \cdot 16$

From a mean of the observations of MM. Delambre and Mechain ....$\Big\}$ 48 50 $13 \cdot 25$

When we employ the same tables of refraction, the mean declination of the pole star for the first of January, 1812............$\Big\}$ 88 18 $18 \cdot 98$

When we compare it with that which M. Delambre had established for 1793 and 1797, M. Mathieu finds the lunisolar precession $50 \cdot 242''$ and $50 \cdot 307''$ or $50 \cdot 274''$ by a mean; if we subtract from it $0 \cdot 2''$ for the planetary disturbances, there will remain $50 \cdot 074''$. The author of the tables of the sun supposed $50 \cdot 1''$ in round numbers. Other astronomers have supposed $50 \cdot 075''$. All this agrees as well as can be desired. However, M. Mathieu has not satisfied himself with this. MM. Biot and Arrago, at Formentera, had employed the elongations of the pole star to verify their latitudes. M. Mathieu made use of the same means. By a mean between the east and west elongations he has only found $0 \cdot 4''$, to subtract from the latitude found by the transit over the meridian. Hence we may suppose $48° 50' 13 \cdot 0''$ for the height of the pole at the southern face of the observatory: a kind of mean which however approaches nearer to the transits.

An inequality of $4 \cdot 95''$ between the east and west elongations furnished him with the means of correcting the right ascension of the pole star. The error of which acted in opposite ways in the two elongations; hence the effect of this error ought to disappear in the sum, but to be double in the difference between the two elongations. To satisfy all these data, we must suppose that the right ascension of the polar star on the 1st of January, 1812, was $0 \cdot 55° 5' 5''$. Thus, by the mere observation of zenith distances, M. Mathieu has verified the height of the pole, its declination, and has made a correction upon its right ascension, which appears slight if we consider the slowness with which that star passes over the meridional wire of the telescope.

The latitude being thus determined, we may with greater confidence endeavour to find the solstitial declination of the sun. The excess of the obliquity calculated by the tables, above that which results

From the winter solstice of 1811, is ... $+ 0 \cdot 86''$

summer solstice of 1812 ...... $+ 0 \cdot 04$

winter solstice of 1812 ...... $+ 1 \cdot 95$

summer solstice of 1813 ...... $+ 0 \cdot 11$

The difference between summer and winter is 1·34″. This can only arise from the refractions which are greater and less certain in winter than in summer. The obliquity drawn from the summer solstice surpasses that of the tables by half that difference. We see that the table of refractions of the board of longitude, makes the solstices agree much better with each other than the other tables; since astronomers of reputation have found as great a difference as 8 or 10″ between the summer and winter solstice. M. Delambre in the 12 solstices which he observed, never found a greater difference than 4″, even with Bradley's tables. The table of refractions which he made, in order to make the two solstices agree with each other, and which had obliged him to reduce the height of the pole to 13″ for the Imperial Observatory, does not differ from that of the Board of Longitude from the zenith to 80°, but by quantities scarcely appretiable, and which depend chiefly upon the thermometrical factor.

The new solstices are four in number. If we suppose that the advantage of the circle of Reichenbach over that of Borda, is as 3 to 1, it will be proper to take the mean between the two determinations, which will reduce almost to nothing the difference between the tables and direct observation. But be that as it may, we see that the height of the pole, the obliquity, and even the fractions, are as well known as can be expected; and that there remain only differences so slight, that we may at pleasure ascribe them either to observation or theory, or divide them equally between both.

These observations of the sun offer likewise a very satisfactory agreement, though not quite so great as that of the stars. In 1811, though the weather was very unfavourable, the difference between the two extremes only once amounts to 3·8″. In June 1812, it only once amounts to 2″; in December, 1812, the weather was so bad, that the difference amounted to 4·6″; and in June, 1813, it was 2·7″.

Account of a planetary Clock, contrived by M. Gerardot.

Account of the Revolutions of the heavenly Bodies, imitated by the Mechanism of Wheels. By M. Janvier.

The first of these machines will excite astonishment, if we consider that the contriver of it had no knowledge of mechanics and astronomy, but what he acquired himself, without any other assistance than a book of the most elementary kind. The machines of M. Janvier are the most perfect that can be produced by a man of science and a consummate artist.

Trigonometrical Operations executed in Holland. By M. the General Krayenhoff.

A great geographical undertaking, executed with all the care and precision employed in the measurement of degrees.

On Spherical Trigonometry. By M. Puissant.

This is an extract of a work which will soon appear, and which

will complete two treatises which the author has already given in geodesy.

On a System of Analytical Formulas. By M. Binet.

On Symmetrical Functions. By M. Gauchy.

On the Means of determining the Number and the Signs of the real Roots of Algebraic Equations. By the same.

These memoirs, of which this is not the place to give an extract, have been approved by the Class, and will appear among those of the *Savans Etrangers.*

Memoirs relative to the Reality and to the Signs of the Roots of Equations. By M. du Bourguet.

This memoir has been approved, and the author invited to simplify some of the considerations which he employs, and to apply them to higher degrees of equations.

Memoir of M. Duvillard on the Theory of Population. Destined likewise to appear in the collection of the *Savans Etrangers.*

On Mills put in Motion by the Reaction of Water, and on a Hydraulic Machine. By M. Manoury d' Ectot.

These mills, from their simplicity and utility, seem often entitled to be preferred to the ordinary mills. As for the machine, the Class was of opinion, as well as the commissioners, that from its extreme simplicity it ought to be ranked among the most interesting conceptions for the arts.

On the Curvature of Surfaces, 4th and 5th Memoirs. By M. Dupin.

On the Experiments to determine the Force, the Flexibility, and the Elasticity of Wood. By the same.

These two memoirs have been considered as deserving a place in the collection of the *Savans Etrangers.* The third, which the author has not yet been able to complete, has, notwithstanding, merited the praises and encouragements of the Class. The author, captain in the first corps of marine engineers, foreign associate of the Institute of Naples, has presented to the Class a printed work, entitled, *Developements in Geometry,* with applications to the stability of vessels, to loading and unloading, to optics, the manuscript of which, in 1812, had obtained the approbation of the Class of sciences, and of which we spoke in our last year's analysis.

(*To be continued.*)

Article XVI.

scientific intelligence; and notices of subjects connected with science.

I. *Chinese Lead.*

Having occasion some time ago for some nitrate of lead, and

having no other lead at hand than a quantity of the sheet lead which comes from China wrapped around tea in the tea-boxes, I employed it for my purpose. I was surprised to find it alloyed with tin, and had the curiosity in consequence to subject it to chemical analysis. The composition of this alloy does not appear quite uniform. One specimen I found composed of

Lead .... 95·8
Tin .... 4·2
                                  ———
                                  100·0

Another consisted of

Lead............................. 95·18
Tin .... 4·82
                                  ———
                                  100 00

We may conclude, with considerable probability, that the quantity of tin added to the lead by the Chinese is 4·5 per cent. The addition of tin to lead renders that metal harder, and gives it more tenacity. The alloy does not seem so liable to tarnish as pure lead. There is another advantage which attends the addition of tin to lead. It renders that metal less injurious when it comes in contact with articles of food; for the tin is always acted upon in preference to the lead.

II. *Native Carbonate of Magnesia.*

Some time ago, when at the India House, I obtained a quantity of a white mineral, partly in lumps and partly in powder, which was distinguished by the name of *native carbonate of magnesia,* from India. I could not get any accurate information respecting the place where it is found, nor who distinguished it by the name of *native carbonate of magnesia.* It has more the aspect of Spanish whiting than of carbonate of magnesia; being destitute of that elasticity which constitutes so remarkable a character in magnesia. It feels light, but not so much so as magnesia. It has not the least resemblance to the native magnesia of Werner. The colour is white, with a very slight shade of yellow. The lumps are all rounded, as if they had been subjected to attrition. The fracture is earthy, and precisely similar to that of chalk. The mineral adheres to the tongue.

On subjecting this mineral to analysis, I found it composed of

Carbonate of magnesia 72
Carbonate of lime 28
                                     ———
                                     100

III. *Vein of Fibrous Limestone in Chalk.*

Some weeks ago I received from Dr. Leach, of the British

Museum, a specimen of chalk from a pit in Kent, containing a vein of fibrous limestone. Dr. Leach had himself observed the vein in the chalk, and had brought away the specimen. The thickness of the vein was about half an inch, and the fibres, as usual, ran across the vein. They were fine, nearly as much so as those of satin spar; but they were destitute of the lustre which distinguishes that variety of fibrous limestone. Its colour is yellowish white; lustre, glimmering. It has not in the least the aspect of calcsinter. Yet the most probable opinion that suggests itself to me respecting its formation is the occurrence of a rent in the chalk, which was gradually filled up by the crystallization of limestone from the water which had made its way through the chalk; for I consider the fibrous form, which limestone veins occasionally assume, as the consequence of crystallization.

On subjecting this fibrous limestone to analysis, I found its composition the same as that of common carbonate of lime.

IV. *Electricity from Cloth.*

(To Dr. Thomson.)

SIR,

On the 6th of March, 1813, I was accidentally left in the dark in the place where the calendar of Mr. Richard Hilton, of Dauven, was at work. I observed a very large quantity of electric fire (so much so as to illuminate the piece) generated at the place where the cloth parted from the paper cylinder; and I afterwards procured sparks in great plenty by the friction of the cloth in passing over some beech staves previous to its entering the calendar; but here it was necessary to damp the piece. The calendar has five cylinders: three of paper, and two of iron, one of which was heated at the time.

As I have not since had an opportunity of observing these appearances, you will oblige me by inserting this note (if you think it deserves a place) as an incitement to some of your friends who have an opportunity to pay a little attention to them.

I am, Sir, yours respectfully,

22, Spring Gardens, Preston. WM. GILBERTSON.

V. *Sudden Change of Temperature from Rain.*

From the theory of latent heat one would be disposed to expect a rise of temperature from the falling of rain; or, at any rate, we should not expect a sudden production of cold in consequence of the occurrence of a shower of rain. On that account I consider the following occurrence as worth recording.

On the 25th of May, 1813, the thermometer in London at half past nine in the morning stood at 60°. Soon after, a heavy shower of rain fell from the west, and lasted half an hour. During its continuance the thermometer sunk rapidly to 50°; and during the whole day never gained its original height at half past nine in the morning; for it rose no higher after the rain than 58°.

VI. *Carbonates of Copper.*

Vauquelin has lately subjected to chemical analysis two species of hydrous carbonates of copper found at Chessy, near Lyons. The first was blue, crystallized, and hard. He found it composed of

Copper	56
Oxygen	12·5
Carbonic acid	25
Water	6·5
	100·0

Now as these numbers do not agree with the doctrine of definite proportions, nor with the previous analysis of the same ore by Klaproth, I think there is reason to suspect some inaccuracy. The proportion of carbonic acid is too great, while that of oxygen is too small.

The green fibrous ore of copper Vauquelin found composed of

Copper	56·1
Oxygen	14·0
Carbonic acid	21·25
Water	8·75
	100·10

This analysis corresponds much better than the preceding with the doctrine of definite proportions. The quantity of oxygen found is correct; that of the carbonic acid I conceive nearly so. The quantity of carbonic acid which really combines with 70 black oxide of copper is 19·257. But unless uncommon precautions are taken, the quantity of carbonic acid driven off from a mineral is apt to be over-rated; because it carries with it some of the liquid from which it makes its escape.

VII. *Precipitation of Copper by Iron and Zinc.*

Vauquelin has published some observations on the method of precipitating copper from its solutions by iron or zinc, which may be useful to the young analyst. I shall therefore notice them. Those chemists who have been in the habit of experimenting on copper must have been aware of the circumstances which he states. Zinc answers better than iron for precipitating copper. Unless the zinc be allowed to remain a sufficiently long time in the solution, the whole copper is not precipitated. Unless there be an excess of acid in the liquid, a portion of the copper is precipitated in the state of oxide. (Ann. de Chim. lxxxvii. 16.) I may add that a portion of zinc always falls in combination with the copper. Therefore the copper, after the liquid is separated, ought always to be digested in dilute muriatic acid, which takes up the zinc without touching the copper.

VIII. *Hjelms and Ekeberg.*

These two Swedish chemists, both possessed of considerable celebrity, died during the course of the last year. I shall take a future opportunity to make the reader acquainted with the chemical works which they published, and the improvements they introduced into the science. Peter Jacob Hjelms was born on the 2d October, 1746; and died on the 7th October, 1813. He first obtained molybdenum in the metallic state. Andrew Gustavus Ekeberg was born in 1767; and died the 11th February, 1813. His experiments on yttria and tantalum are well known.

IX. *Veins of Tin Ore discovered in France.*

Three quartz veins containing tin ore have been recently discovered in the commune of Piriac, in Britanny, near the sea shore, running through a granite mountain. The veins were examined by d'Aubuisson, and the ore was assayed by the Council of Mines at Paris, and found as rich as tin ore usually is. No description of the size or richness of these veins is given. Hence we cannot guess whether they be worth working; but from their being near to Cornwall, situated in the same kind of country, and running in the same direction with the Cornish tin veins, hopes may be entertained that a depot of tin ore may be hereafter discovered in Britanny. See Ann. de Chim. lxxxviii. 162.

X. *Valuable Chemical Library for Sale.*

The Professor of Chemistry at St. Petersburgh, Dr. Scherer, has a most valuable chemical library, which he is desirous of selling. A complete catalogue systematically arranged is printed. The number of volumes is 6060, the greater part German, but also in other languages, as English, French, Italian, Spanish, Russ, Swedish, &c.

Article XVII.

New Patents.

William Moult, Bedford-square, London; for an improved method of acting upon machinery. May 21, 1814.

William Neville, Birmingham, coach brass founder; for a method of making handles, gates, palisades, virandas, balustrades, staircase rails, espalier frames, and various other articles. May 26, 1814.

Grant Preston, Burr-street, London Dock, brazier; for a concavious cabin stove. June 5, 1814.

John Buxton, Great Pearl street, Spitalfields, cotton manufacturer; for an improved method of twisting and laying cotton, silk, and various other articles. June 5, 1814.

Article XVIII.

METEOROLOGICAL TABLE.

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1814.	Wind.	Barometer Max.	Min.	Med.	Thermometer Max.	Min.	Med.	Evap.	Rain.
6th Mo.									
June 10	E				69	42	55·5	—	
11	E		29·70		69	48	58·5	—	—
12	E	29 94	29·70	29·820	74	48	61·0	—	—
13	E				67	55	61·0	—	—
14	W	30·01	29·82	29·910	85	53	69·0	·50	1·00
15	S	29·87	29·75	29·810	78	52	65·0	—	
16	N W	30·00	29·92	29·960	71	43	57·0	—	—
17	N W	30·04	29·92	29·980	68	52	60·0	—	—
18	N W	30·02	—	—				—	—
19	N W	—	29·74	29·880	66	42	54·0	·26	·38
20	N W	29·74	29·58	29·660	65	48	56·5	—	
21	N W	29·83	29·77	29·800	59	49	54·0	—	7
22	N W	30·07	29·83	29·950	60	49	54·5	—	—
23	N W	30·20	30·07	30·135	62	47	54·5	—	2
24	N E	30·27	30·24	30·255	60	41	50·5	—	—
25	N W	30·24	30·04	30·140	63	49	56·0	·30	—
26	N W	30·01	29·99	30·000	62	48	55·0	—	—
27	N W	30·01	29·94	29·975	65	49	57·0	—	8
28	N W	29·94	29·84	29·890	74	47	60·5	—	
29	S	29·86	29 80	29·830	71	55	63·0	·20	
30	N W	29·86	29·81	29·835	74	47	60·5	—	
7th Mo.									
July 1	S W	29·90	29·82	29·860	70	46	58·0	—	
2	N W	29·94	29·90	29·920	74	42	58·0	—	
3	N W	29·95	29·93	29·940	79	50	64·5	·33	
4	N W	29·95	29·94	29·945	75	58	66·5	—	—
5	N W	29·96	29·94	29·950	81	50	65·5	—	6
6	S W	29·96	29·88	29·920	79	49	64·0	—	
7	S W	29·82	29·78	29·800	73	63	68·0	—	—
8	S W	29·78	29 67	29·725	69	62	65·5	—	—
9	S W	29 67	29·67	29 670	72	63	67·5	·50	·13
		30·27	29·58	29·907	85	41	60·01	2·09	1·74

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Sixth Month.—10. A shower in the night. 11. Misty morning, with a smell of electricity in the air : *Cumulus* and *Cirrus* prevail. 12. Showers. 13. A *Stratus* in the meadows : thunder clouds afterwards appeared in the horizon ; and, at 11 p. m., lightning. 14. A thunder storm very early, with much wind, and heavy rain mixed with hail. 15. Wet morning : evening cloudy and windy. 21, 22, 23. Windy, and overcast, with *Cumulostratus* and *Cirrostratus*. A fire in the grate has been again acceptable. 24. A shower early. This evening the cloudy canopy passed off, with a definite boundary to the S., where, at the same time, it appeared to be raining. 25. Again overcast, with *Cumulostratus*, and windy. Much honey dew on the lime-trees. 26. A shower this evening. 27. The same. 28. Cloudy, calm : vane at N. *Cumulostratus*, very heavy in the middle of the day, but which cleared off in the evening. 29. Misty morning : clouds as yesterday. 30. Much sun to-day.

Seventh Month.—1. *Cirrostratus* and *Cirrocumulus*, with some appearance of the *Stratus* at night. 2. a. m. Plumose *Cirri* of uncommon beauty, with *Cumuli* beneath them : the latter prevailed through the day. 3. Clear morning : the *Cumulostratus* again prevails. 4. Large ill-defined *Cirri* fill the sky this morning : various denser clouds succeeded. 5. Rain by half-past six ; then cloudy and fair. At evening the sky cleared, with the wind W., and an orange twilight. 6. Loose driving *Cumuli* : brisk wind : clear twilight, with little colour. 7. Much wind : *Cirrus* passing to *Cirrostratus*, with *Cumulus* beneath : the whole formed to windward. In the evening, a few drops of rain. 8. Wet, a. m. : fair, p. m. : windy night. 9. Windy, cloudy : dripping at intervals. The quickthorn hedges are in many parts quite stript of their leaves by caterpillars.

RESULTS.

Prevailing Wind : North-West.

Barometer : Greatest height . . . . 30·27 inches ;
           Least . . . . 29·58
           Mean of the period . . . . . . 29·907

Thermometer : Greatest height . . . . 85°
           Least 41
           Mean of the period . . . . . . 60·01

Evaporation, 2·09 inches.   Rain, 1·74 inch.

TOTTENHAM,   L. HOWARD.
Seventh Month, 13, 1814.

ARTICLE I.

Biographical Account of Mr. Scheele.   By Thomas Thomson, M.D. F.R.S.

THE transcendent merit of Scheele is so universally known to chemists, and the few incidents in a life devoted to one single object have been so many years published by Crell, that I do not consider myself as able to throw much novelty upon the subject. The chief object which I have in view is to lay before the reader a table of the improvements introduced by this unequalled chemist in every branch of the science : for though his publications are far from voluminous, we are indebted to them for the discovery of a greater number of new chemical agents than to any other chemist that ever existed.

Charles William Scheele was born at Stralsund, the capital of Swedish Pomerania, on the 19th December, 1742.   His father was a tradesman in that city.   He received the first part of his education at a private academy, and was afterwards removed to the public school.   Early in life he showed a strong inclination to learn pharmacy: he was accordingly bound apprentice for six years to Mr. Bauch, an apothecary at Gottenburgh, and continued with him about two years after the term of his indenture had expired.   It was here that he made himself a proficient in chemistry, assisted by his own genius and industry, by the processes performed in the laboratory, and by the facility of making experiments which his situation afforded.

He studied with the utmost diligence the works of Neuman, Lemery, Kunkel, and Stahl.   Neuman's Chemistry was his favourite book, and the book to which, in his own opinion, he was chiefly indebted for his chemical knowledge.   He acquired the habit of experimenting, which is so essential to the chemist, and

which at that time could scarcely be said to exist any where; for chemistry had not yet been reduced to a science, and the method of investigating the chemical properties of bodies, and still more of analysing them, was almost quite unknown. He used to repeat experiments in the night-time out of Neuman and Kunkel, and ascertained several facts which, though previously known to others, were new to him. Thus he found that oil of cloves was set on fire by concentrated nitric acid. On one occasion, while he was engaged in the preparation of pyrophorus, another apprentice, without his knowledge, put some fulminating powder into the mixture. The consequence was a violent explosion; and as the accident happened in the night-time, the family were alarmed, and our young chemist severely reprimanded.

But such accidents rather served to stimulate his zeal for experimenting. His whole soul was devoted to the study of chemistry, and his every thought turned with the most unwearied industry towards its improvement. In short, when he left Gottenburgh, he was probably the best practical chemist of his time.

From Gottenburgh he went to Malmo, to the house of Mr. Kalstrom, an apothecary there. Here he continued two years; and in 1767 went to Stockholm, where he superintended the apothecary's shop of Mr. Scharenberg. In 1773 he exchanged this situation for a similar one in Upsala, in the house of Mr. Loock. He was probably induced to take this step in consequence of the reputation of Bergman as a chemist, and his desire of cultivating his friendship. He had written a dissertation on tartaric acid, containing a great deal of new and curious matter, and he had sent a copy of the manuscript to Bergman. Bergman, totally unacquainted with the name and merit of Scheele, merely looked at the title; and knowing that Margraaf had lately written upon the same subject, and taking it for granted that the manuscript would contain nothing new, he never gave himself the trouble to read it, but returned it to the author without any remark. Scheele, nettled at this treatment, which he considered as contemptuous, sent a copy of the paper to Retzius, Professor at Lund; and Retzius published it in the Stockholm Transactions for 1770, without any mention of Scheele's name.

Scheele's first introduction to Bergman was produced in the following manner:—The celebrated Assessor John Gottlieb Gahn was at that time a student in the University of Upsala, and was greatly distinguished for his abilities and his chemical knowledge. Going one day into Mr. Loock's shop, the master of the shop acquainted him with a circumstance which had occurred to him some days before, and which he could not explain. He had accidentally observed that if vinegar be poured upon nitre that has been exposed to a red heat, fumes of nitrous acid are driven off. Gahn was not able to account for this phenomenon, which appeared inconsistent with the known laws of chemical affinity, but promised to consult Bergman on the subject. Bergman was equally at a loss to explain

the phenomenon. Some days after, Gahn went back to the same shop to relate the ill success of his endeavours to get an explanation of the phenomenon. Here, instead of the master, he found a young man of a dark complexion behind the counter, who told him that the explanation was easy. Nitric acid, said he, like sulphuric, exists in two states. In the first state it has a stronger affinity for potash than vinegar ; but in the second state it has a weaker. It is converted by a red heat from the first state to the second, and therefore is driven off by vinegar.

The young man who gave this luminous explanation was Scheele. Gahn, in consequence, cultivated an acquaintance with him, and continued ever after his most intimate friend, to whom he communicated all his chemical ideas and discoveries. When Gahn proposed to introduce him to Bergman, Scheele mentioned his paper on tartaric acid, and felt indignant at the treatment he had received. Gahn had great difficulty in convincing him that the treatment was entirely owing to inadvertence, and not to any bad intention on the part of Bergman. After the reluctance was once overcome on the part of Scheele, these two illustrious chemists became inseparable friends. Bergman adopted all his opinions, published his discoveries, and even, it is alleged, (though I doubt the accuracy of the statement,) procured him a small annual pension from the Stockholm Academy, to assist him in prosecuting his experiments.

In the year 1775 Mr. Scheele, who had been sighing all his life long for an independent situation, and whose views were exceedingly moderate, settled as an apothecary in the small town of Köping, at the west end of the lake of Malar, upon which Stockholm is situated,—a very small place consisting only of a few wooden houses. The apothecaries in Sweden are under the control of the Medical College, who even limit the number that can exist in any one town. Hence it was only in consequence of the death of an apothecary previously settled at Köping that this vacancy occurred, and it was only in consequence of the appointment of the Medical College that Scheele was enabled to fill it. He purchased from the widow of his predecessor the shop and business, in the year 1777, but continued to live in the same house with her, and the expences of house-keeping were defrayed by them jointly.

Here he prosecuted his chemical experiments with his usual ardour ; and, as he could now devote more of his time to them, he added prodigiously to his discoveries ; and his reputation, which had already spread over the whole civilized world, notwithstanding the disadvantageous situation in which he stood, increased continually every year, and was placed in a still more conspicuous point of view by the puny and unsuccessful attacks that were occasionally made upon him. When he first settled at Köping, his circumstances were not such as to enable him to procure a comfortable room for experimenting in ; and as it was out of his power to give up his darling pursuits, he performed his experiments in an open place, where he was frequently exposed to severe cold. The con-

sequence was a violent attack of rheumatism, which continued progressively to increase till it gradually undermined his constitution ; so that, though he was afterwards enabled to transfer his labours to a more comfortable room, he never recovered his health completely.

About half a year before his death his disorder increased so much that he became sensible that he could not live much longer. This determined him to execute a resolution that he had formed some time before, out of gratitude ; namely, to marry the widow of his predecessor, from whom he had purchased the shop, and with whom he lived. He married her accordingly on the 19th of May, 1786, when he lay on his death-bed. On the 21st he left her by will the whole of his property ; and the same day on which he had so tenderly provided for her he died.

Scheele died in the 44th year of his age, leaving behind him a reputation for genius, industry, and sagacity, which no chemist before him ever acquired, and which probably no successor will ever surpass. The whole bent of his mind was turned towards his favourite science ; his conversation was entirely confined to it ; so that he spoke little, or not at all, in mixed companies. His letters were all upon the same subject ; and his principal correspondent was Assessor Gahn, at Fahlun. I have seen a volume of his letters, which Mr. Gahn has bound up, all devoted to chemical details. He never had the advantage of a liberal education, and was therefore unacquainted with all the modern languages except German, his native tongue. Even of Swedish, though he had lived nearly all his life in the country, he was so imperfect a master, that he never ventured to write in it. His letters, all of them that I have seen, are written in German. His dissertations were written in the same language, and previous to their insertion into the Stockholm Memoirs were translated into Swedish. Hence he laboured under every disadvantage that could retard the progress of a practical chemist. But he was so fortunate in the era in which he made his appearance, his department was so peculiarly his own, he had so few competitors in some of the most brilliant parts of it, that these disadvantages did not prove so injurious to his reputation as they would have done had he lived twenty or thirty years later. Some of his most curious discoveries, indeed, were anticipated by Dr. Priestley ; but a sufficient number were his own, without a competitor, to raise and secure a most brilliant reputation.

I shall now endeavour to lay before the reader a short sketch of all his discoveries, as far as I am acquainted with them, arranged as nearly as possible in the order in which he made them :—

1. The first of his discoveries, published by Retzius, without noticing the original author of it, was the method of procuring tartaric acid—an acid before that time unknown, except as a constituent of cream of tartar.

2. The first dissertation published with his name appeared in the Stockholm Memoirs for 1771, and was upon *fluor spar*. The nature of this substance was at that time unknown, and had even

baffled the sagacity of Margraaf, who had published a dissertation on it some years before. Scheele showed that it could be resolved into lime and a peculiar acid called fluoric acid, the singular properties of which he ascertained and described. In this dissertation he fell into a curious mistake. He thought that silica was a compound of water and fluoric acid. But this mistake was speedily rectified and acknowledged by Scheele himself, who soon found that the silica which appeared in his experiments was derived from the glass vessels corroded by the fluoric acid. Excepting the experiment of Dr. Priestley, little was added to the facts ascertained by Scheele respecting fluoric acid. But within these few years, many new and important facts have been ascertained by the happy sagacity of Gay-Lussac, Thenard, and Davy ; and a new view has been taken of the subject, concerning the accuracy of which chemists have not yet given a decided opinion.

3. Scheele's dissertation on manganese was published in 1774, while he was at Upsala; and I have been told that his experiments had been undertaken at the request of Bergman. I consider it as the most important of all his chemical labours. It contains four discoveries, any one of which would have been sufficient to secure the reputation of the discoverer:—1. That black oxide of manganese is a substance different from every other, and has a strong tendency to unite with phlogiston ; in other respects agreeing with the metallic earths or calces. Hence the inference was easy, and unavoidable, that it is of a metallic nature. Accordingly this inference was immediately drawn by Bergman. 2. That when muriatic acid and black oxide of manganese are heated together, an effervescence takes place, and a gaseous matter passes over, having a yellow colour, and very remarkable properties. This substance he considered as muriatic acid deprived of phlogiston. It was afterwards called oxymuriatic acid : and Davy, who has lately revived the theory of Scheele, has given it the name of chlorine. 3. That there is occasionally mixed with black oxide of manganese an earth hitherto unknown, but the most remarkable properties of which he describes. This is the earth now well known by the name of barytes. 4. That when ammonia is digested upon nitrate of manganese it is totally decomposed. Its phlogiston combines with the manganese, while its other constituent makes its escape in the form of gas. Scheele had, no doubt, examined this gas, since he informs us that it possessed properties very different from carbonic acid gas ; but he gives no description of it. It is now well known to be azotic gas. Hence as this dissertation was published in 1772, and was the result of three years' labour, there can be scarcely any doubt that Scheele was in fact the first discoverer of *azotic gas.*

4. His next paper, published in 1775, was on a new Method of obtaining Benzoic Acid from Benzoin. It consists in boiling powdered benzoin with quick-lime and water, filtering the solution, evaporating it till sufficiently concentrated, and then precipitating the acid by means of muriatic acid.

5. His next dissertation, published likewise in 1775, contains the discovery of arsenic acid, and an account of its properties. He had shown in his dissertation on manganese that phlogiston was one of the constituents of white oxide of arsenic ; the object of the present researches was to discover the other constituent of that substance. This he conceived to be arsenic acid. So complete is this dissertation, that but little has been since added to our knowledge of arsenic acid ; except accurate information respecting its composition, and some improvement in the method of obtaining it.

6. It seems to have been about this time that Scheele discovered oxalic acid by heating a solution of sugar in nitric acid. Bergman's dissertation on it made its appearance in 1776, as an inaugural dissertation ; and as no notice was taken in it of Scheele, Bergman passed for some time as the real discoverer. But the truth was gradually divulged, and has been long universally known. Indeed, I put the question to Assessor Gahn, who was at that time a student at Upsala, and in terms of friendship both with Bergman and Scheele ; and he assured me that Scheele was the person who discovered oxalic acid. I regret that I did not ask for some information respecting the history of the discovery. But my stay at Fahlun was so short, and I had so much to ask from a man of Gahn's general information of the interesting persons with whom he had been in habits of intimacy, that many material points escaped my recollection at the time.

7. Scheele's first dissertation after his removal to Köping was upon the Nature of Silica and Alumina, both of which he showed, contrary to the opinion of Beaumé, to be peculiar earths, not capable of being transmuted into each other, or into any other known earths.

8. His next dissertation was published in 1776, in the Memoirs of the Stockholm Academy, as indeed was the case with all the preceding. It was the analysis of a urinary calculus, which he showed to consist of a peculiar acid, the properties of which he described with his usual minute accuracy. This is the acid now well known by the name of *uric acid*. This paper was the commencement of our knowledge of urinary calculi, which has been since carried to so great a height by succeeding chemists.

This dissertation contains likewise some very important remarks on the constituents of urine, and may be considered as the first approach to an analysis of that very complicated liquid.

9. Scheele's next publication was his work on Air and Fire, which was first printed in 1777, though from the prodigious number of experiments which it contains it must have cost him many years of labour. In every point of view it must be considered as one of the most extraordinary works that ever appeared ; and contains a vast number of curious discoveries, some of which had been anticipated by Dr. Priestley ; but the greater number were new, even when the work On Air and Fire appeared. The object of the work was to show that *heat* is a compound of *oxygen* and *phlogiston*,

light of oxygen and a *smaller quantity of phlogiston.* To attempt
a bare enumeration of all the curious discoveries contained in this
small volume would be out of the question. I shall just notice some
of the most remarkable.   When black oxide of manganese is
exposed to a red heat, or heated with sulphuric acid, it gives out a
species of air the properties of which are described.   Scheele calls
it *empyreal air.*   It is the substance now well known under the
name of *oxygen gas,* and had been discovered three years before
the publication of Scheele's work by Dr. Priestley.   Atmospherical
air is composed of oxygen gas and another air, the properties of
which are described.   It is the gas now well known under the name
of *azote.*   Combustion deprives air of its oxygen ; so do the sul-
phurets, and a mixture of iron filings and sulphur.   The metallic
oxides, when reduced, give out oxygen gas.   In short, the proper-
ties of oxygen gas, and its action on different substances, are
described with so much minuteness, and general accuracy, that this
work must have furnished Lavoisier with the greatest part of the
facts which he employed in establishing his system.   The experi-
ments on nitrous gas, sulphureted hydrogen gas, fulminating gold,
pyrophorus, are no less curious and important : nor ought I to omit
mentioning the curious facts ascertained respecting the action of the
prismatic rays on the muriate of silver, and respecting the radiation
of heat.   In short, this book contains a complete theory of chemis-
try, such as Scheele had deduced it from his experiments.   The
extreme simplicity of his apparatus, and the consequent mistakes
into which he fell in some material points, prevented his deductions
from being so accurate as might have been expected from so careful
an experimenter as Scheele.   The theory was in consequence erro-
neous ; but the materials of which it was composed are entitled to
our highest admiration, and probably could not have been put
together by any other man than Scheele.

10. Neither his dissertation on a new method of procuring
calomel, nor that on a new method of procuring powder of algaroth,
both published in 1778, is entitled to particular attention.   They
describe processes which were probably useful to the apothecary,
but which contributed little to the improvement of the science.

11. But his paper On Molybdena, published likewise in 1778,
was another master-piece of chemical analysis.   He points out the
difference between plumbago and molybdena, substances which had
hitherto been confounded, and shows that molybdena consists of
sulphur united to a peculiar acid, since called molybdic acid, the
properties of which he describes.

12. The same year he published his method of preparing the
pigment now well known by the name of *Scheele's green.*   It is an
arsenite of copper, and was formed by precipitating sulphate of
copper by arsenite of potash.

13. During the whole of the year 1778 he continued a set of
experiments in order to determine the proportion of oxygen gas in
the atmosphere. His method was to deprive a certain portion of the

air of its oxygen by means of a mixture of iron filings and flowers of sulphur. The result was, that air contains always almost exactly the same proportion of oxygen, and that this proportion amounts to $\frac{3}{11}$, or about 27 per cent. I had the curiosity to repeat these experiments many years ago, and found that the diminution of bulk varied according to circumstances, sometimes amounting to 33 per cent. The reason is that hydrogen is evolved, and ammonia formed, at the expense of the azotic portion of the air; so that when the experiment is made in this way, the diminution is in all cases greater than the proportion of oxygen contained in the air examined. It was this experiment of Scheele that misled chemists respecting the proportion of oxygen in common air: and when Cavendish rectified the mistake, in 1782, it was some time before the requisite attention was paid to his results; not indeed before Berthollet obtained similar results in Egypt.

14. In 1779 he published a curious set of experiments on the decomposition of the salts having soda for a base, by means of iron or quick-lime, when deposited in a cellar. He found that when a plate of iron was dipped into a solution of common salt, and suspended in a cellar, after an interval of a fortnight there was an efflorescence of carbonate of soda upon the iron. The same thing happened if sulphate or nitrate of soda were substituted for common salt. When any of these salts was mixed with quick-lime slightly moistened, and left in the same situation, the same decomposition took place. These curious facts have not hitherto been explained in a satisfactory manner, unless we admit the effect of quantity so ingeniously advanced by Berthollet in his treatise on affinity.

15. The same year he published his experiments on plumbago, in which he demonstrated that it possessed the properties of charcoal, but contained about $\frac{1}{10}$th of its weight of iron.

16. His curious paper on milk made its appearance in the Memoirs of the Stockholm Academy for 1780. He ascertained the chemical properties of curd, showed upon what the coagulation of milk depended, and proved that it contained a new acid of a peculiar nature, to which the name of *lactic acid* has been given. This acid does not crystallize, cannot be distilled over, and forms deliquescent salts with all the alkaline and earthy bases The French chemists endeavoured to prove that it is merely acetic acid holding some saline matter in solution. But Berzelius has shown that their opinion is ill founded, and that lactic acid possesses peculiar properties quite different from those of acetic acid.

17. His next discovery, published likewise in 1780, was that of *saclactic acid*, which he obtained by digesting a solution of sugar of milk in nitric acid. The peculiar nature of this acid was denied at first by some German chemists; but their proofs were quite insufficient to overturn the accurate experimental conclusions of Scheele.

18. Scheele's memoir on tungsten was published in the Memoirs of the Stockholm Academy for 1781, He showed that it was a

compound of lime and a peculiar acid, to which the name of tungstic acid was given. The Elhuyarts afterwards showed that Scheele's tungstic acid was a triple salt which had been formed during his experiments, and that the true tungstic acid is an insoluble tasteless powder. They showed also that it is a metallic oxide, and even succeeded in reducing it to the metallic state.

19. In 1782 he published his observations on ether, containing the result of a prodigious number of experiments, made partly with the view of ascertaining the various acids and other substances capable of converting alcohol into ether, and partly in order to determine the nature of the change produced upon alcohol when it is converted into ether. It would be impossible to state all the facts contained in this curious paper, without transcribing almost the whole of it. The general result was, that ether is dephlogisticated alcohol. This gave origin to the French theory, which prevailed for some time, that ether is alcohol united to oxygen. But as facts are utterly inconsistent with this opinion, it has been for some years altogether discarded.

20. In the same year he published a method of preserving vinegar (obtained from grain) for a long time without undergoing decomposition. The method was to expose it to a boiling heat, and then to cork it up.

21. Scheele's two dissertations on *Prussian blue* were published in the Memoirs of the Stockholm Academy for 1782 and 1783. They place in a very conspicuous point of view the patient industry of the author, his invincible perseverance, uncommon sagacity, and unrivalled skill in the knowledge of the chemical action of bodies on each other. Though Prussian blue had been known for 70 years, and had been subjected to many experiments by chemists of acknowledged abilities, nothing was known respecting its constituents. Scheele ascertained its composition, separated the prussic acid, ascertained its properties, determined its composition, and showed in what way it acts upon other bodies. Very little has been since added to these admirable dissertations, if we except the important discoveries of Mr. Porrett, which are not yet published.

22. In 1784 he published an account of a sweet-tasted matter which separates from oils when they are digested with litharge. This substance has been since known by the name of the *sweet principle of oils.*

23. The same year he gave the method of obtaining citric acid in a state of purity ; which had not been hitherto done. Lemon juice was saturated with chalk, the precipitate washed, and mixed with as much diluted sulphuric acid as is just sufficient to saturate the lime employed. The liquid is then filtered, concentrated, and set by to crystallize.

24. His next discovery was, that rhubarb contains a considerable quantity of oxalate of lime. He separated it by digesting powdered rhubarb in water, and pouring off the liquid, which carried off the vegetable matter partly in solution, partly suspended, while the

earthy salt remained behind. In a subsequent paper he gave a list of a considerable number of vegetable substances, chiefly employed in medicine, from which he extracted the same oxalate.

25. His next discovery was the analysis of the salt of wood sorrel, well known in this country under the name of essential salt of lemons. He showed it to be a salt composed of oxalic acid and potash. It owes its acid qualities to the quantity of acid which it contains. It is a binoxalate.

26. In 1785 he published a dissertation on the separation of magnesia from Epsom salt. When common salt and sulphate of magnesia are mixed, a double decomposition takes place ; and if the liquid be set aside for some days in winter, when the thermometer is below the freezing point, the sulphate of soda crystallizes, and may be separated, after which the magnesia may be precipitated in the usual way by an alkaline carbonate.

27. The same year he demonstrated that the siderite, or supposed new metal extracted by Bergman from cold short iron, was phosphuret of iron ; and that Proust's perlated salt, extracted from urine, was phosphate of soda.

28. His experiments on the acid contained in fruits and berries, published likewise in 1785, contains a vast number of new facts. He points out the method of procuring malic acid, and determines its properties. Some fruits, as cranberries, whortleberries, birdcherries, and the berries of night-shade, contain only citric acid ; some, as barberries, elderberries, sloes, serviceberries, plums, contain only malic acid ; while others, as gooseberries, currants, bilberries, cherries, strawberries, cloudberries, raspberries, contain nearly equal quantities of both acids. In the same dissertation he showed that malic acid is formed when sugar is treated with nitric acid at the same time with the oxalic acid ; that gum, sugar of milk, starch, and many other substances, yield both acids when digested in nitric acid. In short, this most valuable paper contains almost every thing relating to the vegetable acids, which was afterwards spread out by Fourcroy, and some other chemists, with so much parade, as if they had been novelties discovered by themselves.

29. The last paper which Mr. Scheele published, as far as I know, contains his discovery of gallic acid. Unless my memory deceives me, for I have it not in my power at present to consult the original, it made its appearance in 1786, and not till after the death of the author. He had set aside an infusion of nutgalls for some time in an open vessel. Crystals formed at the bottom of the vessel. These he separated and examined, and found them to consist of a peculiar acid, to which the name gallic acid has been given, the properties of which he describes.

Such, omitting a few controversial papers, and some letters to Crell of minor importance, is a list of Scheele's writings and discoverie , as far as I am acquainted with them. Though far from a voluminous writer, his discoveries, both in point of number and

importance, are superior to those made by any other chemist that has hitherto appeared. It will serve, perhaps, to convey a clearer idea of this truth, if I give a list of the new substances for the knowledge of which we are indebted to his fortunate sagacity :—

1. Tartaric acid.	10. Molybdic acid.
2. Fluoric acid.	11. Lactic acid.
3. Chlorine.	12. Tungstic acid.
4. Barytes.	13. Prussic acid.
5. Arsenic acid.	14. Sweet principle of oils.
6. Oxalic acid.	15. Citric acid.
7. Uric acid.	16. Malic acid.
8. Azotic gas.	17. Gallic acid.
9. Sulphureted hydrogen gas.	

Thus he discovered no fewer than 17 new chemical substances; the greater number of which are at this day of indispensable utility to the chemist, and act a most important part in the phenomena of nature. I have omitted in the above enumeration various important discoveries, that of oxygen in particular; because Scheele, though he undoubtedly had the merit of discovering that substance without any assistance from others, was not the person who first made it known to the world. I have omitted likewise noticing the new methods of experimenting which he first introduced, and the precision at which he always aimed, and which he generally attained. This had no little effect upon the science, and contributed in no small degree to form that analytical skill which has now become pretty generally diffused among chemists. In short, if we take into consideration that Scheele died when still a young man, that he was destitute of a liberal education, that he spent his life in a foreign country, with the language of which he was never sufficiently acquainted to be able to write it, and that the greatest part of his career was passed in the situation of an apothecary's assistant, without any house or laboratory of his own—if all this be taken into view, and compared with the number and splendour of his discoveries, it will be admitted, I think, without hesitation, that he was one of the most extraordinary men that ever existed, and one of the brightest ornaments of chemistry and of human nature.

ARTICLE II.

On the Oxides of Arsenic. By Thomas Thomson, M.D. F.R S.

THE first person who made a satisfactory set of experiments to determine the proportions in which arsenic unites with oxygen was M. Proust. According to his experiments, which were published

in the Journal de Physique, the white oxide of arsenic is composed of

        Arsenic 100
        Oxygen .... 32·979

And arsenic acid of

        Arsenic 100
        Oxygen .... 52·905

These experiments were afterwards repeated by Rose, whose accuracy is well known, and he obtained precisely the same results. Bucholz also made experiments on the subject, and came nearly to the same conclusions.

Thenard, in his paper on nickel, published in the 50th volume of the Annales de Chimie, gives us a set of experiments which he made to determine the constitution of arsenic acid, in order to enable him by means of it to ascertain the quantity of arsenic which existed in certain ores that he was analysing. He informs us that he acidified 100 grains of arsenic by means of nitric acid, and that he found 100 parts of arsenic acid to be composed of 64 arsenic + 34 oxygen. According to this statement, arsenic acid is composed of

        Arsenic 100
        Oxygen .... 56·25

He informs us farther, that 100 parts of white oxide of arsenic when converted into arsenic acid become 116 parts, and of course combine with 16 parts of oxygen. According to this statement, white oxide of arsenic is composed of

        Arsenic 100
        Oxygen .... 34·694

Still more lately Berzelius made a set of experiments on the same subject, assisted by his peculiar ideas respecting the ratio existing between the oxygen of the acid and the base of salts. These experiments were published in the 80th volume of the Annales de Chimie. He converted 100 parts of arsenic into acid by means of nitric acid, evaporated the liquid to dryness to get rid of the nitric acid, re-dissolved it in water, and put the solution into a platinum crucible together with a solution of a determinate quantity of oxide of lead dissolved in nitric acid. This mixture was evaporated to dryness, and then heated to redness. Different experiments made in this way gave him arsenic acid composed of 100 arsenic combined with 48·3, 49·5, and 53 of oxygen. These experiments, though they do not coincide exactly with each other, yet agree sufficiently with those of preceding chemists to confirm their results, and to satisfy us that arsenic acid must be a compound

of 100 metal with about 50 oxygen. Other experiments made by Berzelius served to confirm this conclusion. He found the arsenite of lead to be a compound of 100 arsenious acid + 118·977 yellow oxide of lead. Now 118·977 parts of this oxide contain 8·5068 parts of oxygen; and 8·5068 × 3 = 25·5204; which number, according to Berzelius' law, indicates the quantity of oxygen in 100 arsenious acid; for in the arsenites the acid contains thrice as much oxygen as the base According to this mode of reasoning, the white oxide of arsenic is composed of

Arsenic ....74·48......100
Oxygen ....25·52...... 34·263

Arseniate of lead, according to the analysis of the same accurate experimenter, is composed of

Arsenic acid ....100·0
Yellow oxide of lead ....237·5

Now in the arseniates the acid contains twice as much oxygen as the base: 237·5 parts of yellow oxide of lead contain 16·981 parts of oxygen; and 16·981 × 2 = 33·962. Therefore arsenic acid is composed of

Arsenic ....66·038......100
Oxygen ....33·962...... 51·428

A conclusion which agrees sufficiently well with the analyses of preceding experimenters.

Such were the experiments hitherto made on the combination of arsenic with oxygen. They seemed to leave no doubt that the quantity of oxygen in the white oxide was to that in arsenic acid as two to three : and I considered it as established in my table published in the *Annals of Philosophy,* ii. 42, that white oxide of arsenic consisted of 100 metal + 33·333 oxygen, and arsenic acid of 100 metal + 50 oxygen.

But Berzelius has lately turned his attention again to this subject, and published a new set of experiments, in which he has corrected his former ones; and he has drawn very different conclusions with respect to the composition of arsenious and arsenic acids. (See *Annals of Philosophy,* iii. 93.) He found by these experiments that arseniate of lead is composed of

Arsenic acid 100
Yellow oxide of lead ....194·11

And arseniate of barytes of

Arsenic acid 100
Barytes ....132·70

Now 194·11 yellow oxide of lead contain 13·878 oxygen, and 132·70 barytes contain 13·89 oxygen. These quantities agree

sufficiently well with each other; but whether we multiply them by two or by three, they will not give us a quantity of oxygen corresponding with the constitution of arsenic acid as previously determined by experiment. Therefore if that previous determination be accurate, it follows that the law of Berzelius respecting the quantity of oxygen in the acid being a simple multiple of the oxygen in the base, does not hold with the arseniates.

This deviation from the law induced him to consider the previous determination of chemists as erroneous. To decide the point experimentally, he mixed five parts of arsenious acid with 20 parts of sulphur in a small retort. By the application of heat, the sulphur absorbed the oxygen from the acid, and made its escape in the form of sulphurous acid. The loss of weight was 3·05 parts, which Berzelius considered as sulphurous acid. Now 3·05 of sulphurous acid contain, according to Berzelius' estimate, 1·5185 of oxygen. This therefore must have been the quantity of oxygen contained in five of arsenious acid. Hence 100 parts of arsenious acid contain at least 30·37 parts of oxygen; so that it is composed of

Arsenic ....100·000
Oxygen .... 43·616

This result he considers as fully confirming the suspicion deduced from the analysis of the arseniates, that both arsenious and arsenic acids contain a greater proportion of oxygen than has been hitherto supposed. Arsenic acid, he conceives, must contain thrice the quantity of oxygen in the base with which it combines. But we have seen that arsenic acid combines with a quantity of yellow oxide of lead which contains 13·878 oxygen. Now 13·878 × 3 = 41·634. Therefore 100 arsenic acid must contain 41·634 oxygen. It is therefore composed of

Arsenic 100
Oxygen .... 71·333

By more exact experiments than those published in the Annales de Chimie, above referred to, he found that arsenite of lead is composed of

Arsenious acid 100
Yellow oxide of lead ....111·17

Now 111·17 yellow oxide of lead contain 7·95 oxygen, and 7·95 × 4 = 31·8. This nearly coincides with the analysis of arsenious acid, and shows that in the arsenites the acid contains four times the quantity of oxygen that exists in the base.

Berzelius likewise made a set of experiments to discover, if possible, the existence of an oxide of arsenic containing less oxygen than arsenious acid. He distilled a mixture of pounded arsenic and calomel in a retort, and obtained a brown mass bordering upon yellow, which he considered as a triple muriate, composed of

muriatic acid united with black oxide of mercury, and an oxide of arsenic containing less oxygen than arsenious acid. It seems to have been a mixture of calomel and chloride of arsenic.

Such are the new experiments of Berzelius, by which he considers himself as warranted to set aside the conclusions drawn from his own former experiments, and from those of Proust, Rose, Bucholz, and Thenard, which all correspond very well with each other. But I do not consider these new experiments, however carefully they may have been made, as capable of refuting the former experiments on the composition of arsenic acid. These experiments are in themselves extremely easy and simple, and one cannot see any source of ambiguity or fallacy that would account for so great an error as that of nearly 15 per cent. in the weighing, which must be supposed if Berzelius' new determination be admitted as correct. To be able to judge with some precision of the nature of this experiment, I repeated it myself in the following manner:—I put 100 grains of metallic arsenic into a retort, and converted it into arsenic acid by means of nitric acid. I then distilled off the nitric acid, and exposed the dry arsenic acid in the bottom of the retort to the greatest heat which I could raise by means of an Argand's lamp; and this heat was continued till the retort became perfectly dry. The retort was then weighed. It was found that the weight of the arsenic had increased from 100 grains to 152·4 grains. Now in this experiment, which I presume is similar to what had been previously done by Proust, Rose, Bucholz, and Thenard, I do not perceive any source of fallacy. The arsenic was merely dissolved in the acid, and the acid distilled off into a receiver; and, for greater security, what came over into the receiver was examined, but no traces of arsenic acid, or of arsenic in any state, could be found in it. No arsenic therefore was lost; nor can any source of loss be pointed out; no filter was employed; nothing, in short, which could account for the loss of 19 grains, the quantity wanting to make up the weight of the arsenic acid, according to the conclusion of Berzelius; nor can it be said that the arsenic was not wholly converted into arsenic acid; for I dissolved it in water, and found a residue of white oxide of arsenic weighing exactly 0·5 grain. Now this small deficit, which does not exceed 0·06 grain of oxygen, I added to the weight of the arsenic acid found; for the weight of the acid as I first took it was only 152·34 grains.

It is by no means unlikely that the heat of an Argand's lamp was not sufficient to free the arsenic acid from the whole of its water; but if we suppose that any water adhered to it, this makes still farther against the conclusion of Berzelius; for in that case the true increase of weight was less than I found it. As therefore the experiments made by myself and others to convert arsenic into arsenic acid are exceedingly simple and easy, and as they are quite inconsistent with the new determination of Berzelius, I do not see any other alternative at present than to conclude that this ingenious

and indefatigable chemist has somehow or other deceived himself, and that the results which he has obtained are not correct.

If we were to consider the result of my experiment as coming nearer the truth than those of preceding experimenters, it would follow from it that arsenic acid is composed of

Arsenic 100
Oxygen .... 52·4

And supposing the oxygen in arsenious acid to that in arsenic acid as two to three, which there can be no doubt is really the case, arsenious acid must be composed of

Arsenic 100
Oxygen......................... 34·93

Now this agrees very nearly with the determination of Thenard, who found that 100 arsenious acid required 16 oxygen to be converted into arsenic acid, and hence concluded that it is composed of arsenic 100 + 34·694 oxygen. The constituents of arsenic acid as here stated coincide very nearly with the determination of Proust, who found it composed of 100 arsenic + 52·905 oxygen. These coincidences give me some confidence in the accuracy of my determination. Were we to put any confidence in it, an atom of arsenic would weigh 5·726, which is somewhat less than the weight assigned in my original table. I cannot say that the evidence adduced by Berzelius for the existence of two new oxides of arsenic appears to me satisfactory. His reason for admitting the oxide supposed to be obtained by subliming arsenic and calomel is little more than analogy. At any rate, as the oxide was not obtained in a separate state, its existence cannot be considered as put beyond doubt. As to the supposed black oxide obtained by exposing arsenic to the open air, for any thing that appears to the contrary, it may be a mixture of metallic arsenic and white oxide of arsenic. It appears to me at present, from all the facts with which I am acquainted, that arsenic in its oxidation is similar to sulphur, phosphorus, iron, nickel, and cobalt. It combines with two proportions of oxygen, bearing to each other the ratio of two to three. Whether a protoxide of arsenic exists does not at present appear, though it is by no means unlikely.

ARTICLE III.

Account of Dr. Wollaston's Scale of Chemical Equivalents.*

THIS scale is of so much convenience to the practical chemist,

* Dr. Wollaston's paper is published in the Philosophical Transactions for 1814, part i.

Fig. 1. *Fig. 2.* *Fig. 3.*

CHEMICAL EQUIVALENTS

A. Wollaston's Scale of Chemical Equivalents
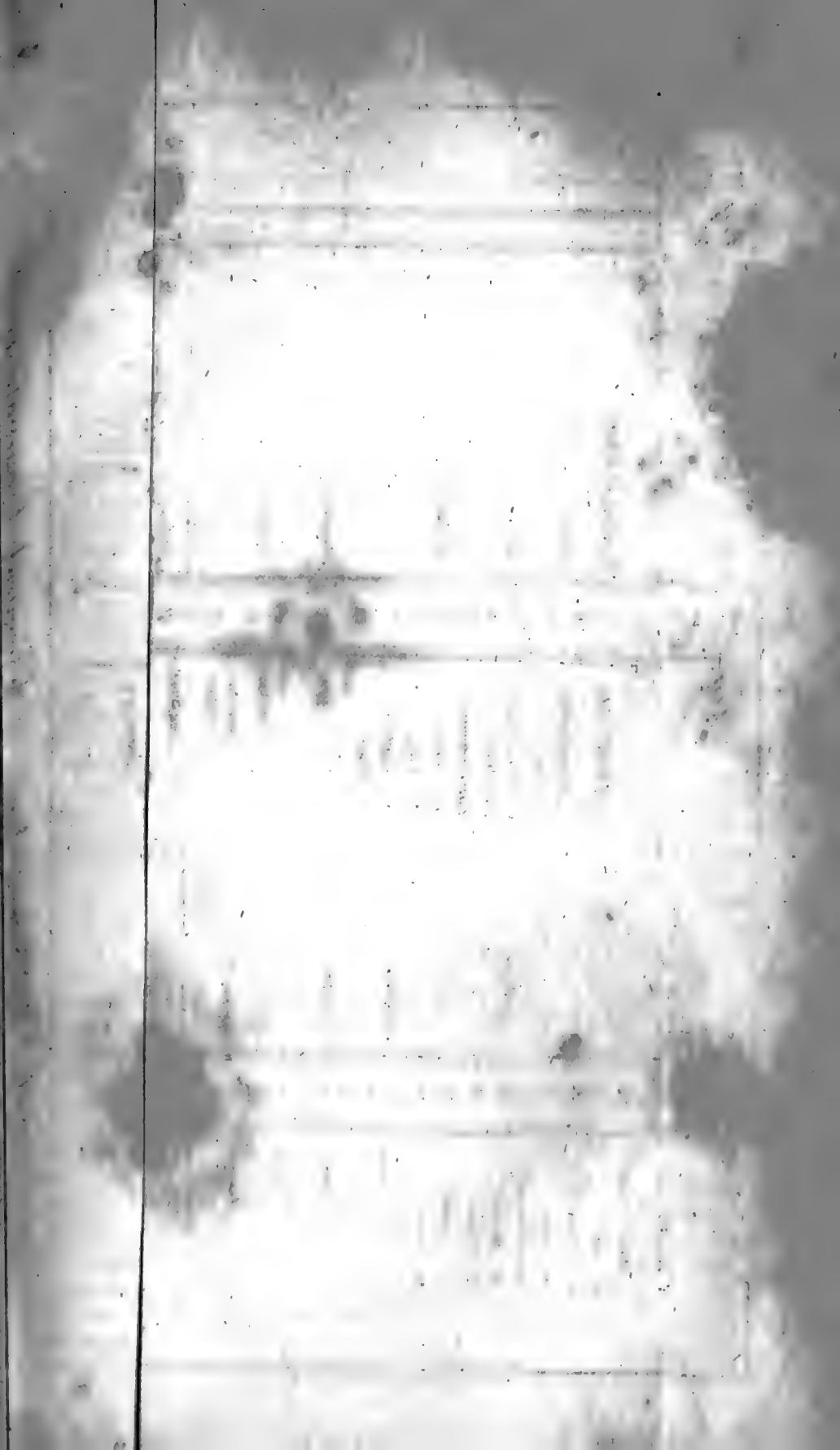
that I seize the earliest opportunity to make it known to my readers in general. It gives the composition of any weight whatever of any of the salts contained on the scale, the quantity of any other salt necessary to decompose it, the quantity of new salt that will be formed, and many other similar things, which are perpetually occurring to the practical chemist, and cannot be answered without an arithmetical calculation. I have used such a scale for above six months, and found it attended with numberless advantages. I cannot undertake to explain the instrument, either more clearly, or in fewer words, than Dr. Wollaston has himself done in his own paper. I conceive, therefore, that the best thing I can do is to present my readers with the following extract from that paper, which contains the most material part of it, as far as the explanation of the instrument is concerned:—

" It is not my design, in the table which follows this paper, to attempt a complete enumeration of all those elements or compounds which I suppose to be well ascertained, but merely to include some of those which most frequently occur. I do not offer it as an attempt to correct the estimates that have been formed by others, but as a method in which their results may be advantageously applied in forming an easy approximation to any object of our inquiries.

" The means by which this is effected may be in part understood by inspection of the Plate [XXII.], in which will be seen the list of substances intended to be estimated, arranged on one or the other side of a scale of numbers in the order of their relative weights, and at such distances from each other, according to their weights, that the series of numbers placed on a sliding scale can at pleasure be moved, so that any number expressing the weight of a compound may be brought to correspond with the place of that compound in the adjacent column. The arrangement is then such, that the weight of any ingredient in its composition, of any reagent to be employed, or precipitate that might be obtained in its analysis, will be found opposite to the point at which its respective name is placed.

" In order to show more clearly the use of this scale, the Plate exhibits two different situations of the slider, in one of which oxygen is 10, and other bodies are in their due proportion to it, so that carbonic acid being 27·54, and lime 35·46, carbonate of lime is placed at 63.

" In the second figure, the slider is represented drawn upwards till 100 corresponds to muriate of soda; and accordingly the scale then shows how much of each substance contained in the table is equivalent to 100 of common salt. It shows, with regard to the different views of the analysis of this salt, that it contains 46·6 dry muriatic acid, and 53·4 of soda, or 39·8 sodium, and 13·6 oxygen; or if viewed as chloride of sodium, that it contains 60·2 chlorine, and 39·8 sodium. With respect to reagents, it may be seen that 283 nitrate of lead, containing 191 of litharge employed to sepa-

rate the muriatic acid, would yield a precipitate of 237 muriate of lead, and that there would then remain in solution nearly 146 nitrate of soda.   It may at the same time be seen, that the acid in this quantity of salt would serve to make 232 corrosive sublimate containing 185·5 red oxide of mercury, or would make 91·5 muriate of ammonia, composed of 6 muriatic gas (or hydromuriatic acid) and 29·5 ammonia.   The scale shows also, that for the purpose of obtaining the whole of the acid in distillation the quantity of oil of vitriol required is nearly 84, and that the residuum of this distillation would be 122 dry sulphate of soda, from which might be obtained, by crystallization, 277 of Glauber salt containing 155 water of crystallization. These and many more such answers appear at once by bare inspection, as soon as the weight of any substance intended for examination is made by motion of the slider correctly to correspond with its place in the adjacent column.

" With respect to the method of laying down the divisions of this scale, those who are accustomed to the use of other sliding-rules, and are practically acquainted with their properties, will recognise upon the slider itself the common Gunter's line of numbers (as it is termed), and will be satisfied that the results which it gives are the same that would be obtained by arithmetical computation.

" Those who are acquainted with the doctrine of ratios, and with the use of logarithms as measures of ratios, will understand the principle on which this scale is founded, and will not need to be told that all the divisions are logometric, and consequently that the mechanical addition and subtraction of ratios here performed by juxta-position, corresponds in effect to the multiplication and division of the numbers by which those ratios are expressed in common arithmetical notation.

" To others who are not equally conversant with the nature of logarithms, and consequently have not so correct a conception of the magnitudes of ratios, some further explanation of the mode in which the scale of equivalents is constructed, will, I presume, be acceptable.

" They will observe, that the series of natural numbers are not placed at equal intervals on the scale ; but that at all equal intervals are found numbers which bear the same proportion to each other. In fig. 3, some of the larger intervals alone are represented on a line similarly divided.   The succession of intervals, marked A, B, C, D, E, are all equal, and at these points of division are placed numbers 1, 2, 4, 8, 16, which increase progressively by the same ratio.   And since the series 3 : 6 : 12 : 24 increase in the same ratio of 1 to 2, these intervals *a, b, c, d, e,* are the same as the former.   At another succession of different yet equal intervals, marked F, G, H, I, are placed numbers 1, 3, 9, 27, which increase regularly by an equal ratio of 1 to 3 ; and by means of a pair of compasses it would be found that the interval from 2 to 6, or from 6 to 18 (which are in the same ratio of 1 to 3), is exactly equal to **F G,** the interval between 1 and 3.   As any single space represents

any one ratio, so the sum of any two or three equal spaces represent a double or triple ratio. If 1 be increased three times by the ratio of 1 to 2, it becomes 8, which bears to 1 triple the ratio of 2 to 1. This ratio is therefore rightly represented by A D, which is the triple of A B.

" The distances of the intermediate numbers 5, 7, 10, 11, 13, &c. from 1 are likewise made proportional to the ratios which they bear to 1, and are easily laid down by means of a table of logarithms; for as these are arithmetic measures of the ratios which all numbers bear to unity, the spaces proportional to them become linear representations of the same quantities.

" As the entire spaces A D, A E, represent the ratios of 8 and of 16 respectively to 1, so the difference D E represents the ratio of 8 and 16, which stand at D and E, to each other. And in the same manner, any other space, *k l*, represents correctly the ratio of 7 to 13; so that the measure of a fraction expressed by quantities that are incommensurate is rendered as obvious to sight as that of any simple multiple. And if a pair of compasses be opened to this interval, and transferred to any other part of the scale, the points of the compasses will be found to rest upon numbers bearing the same proportion to each other as those from which the interval was transferred.

" It is exactly in this manner that the various points in the column of equivalents indicate the several quantities sought in any given position of the slider. The relative distances, at which the articles are placed, represent so many different openings of the compasses rendered permanent and presented to view at once. In the table, which I shall place at the end of this communication, the relation of the various substances enumerated to each other is expressed by numbers. In the engraved scale of equivalents, the ratios of these numbers are represented by logometric intervals at which they are placed, their several positions being determined by those of their respective numbers on the slider, which is logometrically divided. Consequently all the several points in the column of equivalents will indicate numbers in the same due proportions to each other, whatever part of the scale may be presented to them. Those who seek information may obtain it by inspection; those who already possess it may be able to correct the positions of some articles by direct comparison with the best analyses upon record, in whatever numbers the results of those analyses may happen to be expressed."

Article IV.

Experiments to determine the Constituents of Azote.
By John Miers, Esq.

(To Dr. Thomson.)

sir,

The consideration of the nature of azote has been, from the very first period of its discovery, a subject of continual controversy ; and to the present moment chemists are yet divided in their opinions as to its real nature. The earlier experiments of Priestley, of the German and the Dutch chemists, those of Girtanner, with the refutations of them by Berthollet and Lagrange, the more recent ones of Allen and Pepys on respiration, and the delicate researches of Sir Humphry Davy, have all tended to the alternate proval and disproval of the compound nature of azote.

Goettling, Crell, and Wigleb asserted, that on passing water in a state of vapour through ignited tubes, they found it converted into azote ; they hence concluded azote to be compounded of water and caloric. Van Troostwyck, Dieman, Van Hausch, Van Mons, &c. denied the truth of this asserted conversion of water into azote, accounting for the appearance of the latter by supposing that atmospheric air must have passed through the interstices of the earthen tubes. Girtanner repeated these experiments of the German chemists, and confirmed their assertion of the formation of azote from water ; but accounted for its appearance by supposing the water to be deoxidated, a portion of its oxygen having been attracted by the argillaceous earth of the tube. He hence concluded azote to be a compound of hydrogen and oxygen, in which the latter existed in a less proportion than in water, or in other words, that " azote is water deprived of part of its oxygen." The air of certainty with which these experiments were published roused the attention of Berthollet, who, in concert with Lagrange, repeated them with every precaution recommended by Girtanner ; but, after the most strict research, they could not find the least appearance of azote. As this assertion of the Germans thus received the most positive contradiction by two chemists so justly famed for their accuracy of investigation, it naturally resulted that the indecisive opinions of chemists was determined in favour of the simple nature of azote. Recurring, however, to the experiments of Girtanner, it is possible that atmospheric air may have introduced itself through the pores of the earthen tubes, as supposed by Berthollet and Lagrange : but it is also possible, from the experiments which I have now to detail, that it was really formed in the process, and not derived from without. Be this as it may, the subsequent silence of Girtanner, the known rhapsodical style of his

writings, and the extravagance of his hypothesis, served only the
more to convince the chemical world of the falsity of his assertions.
If it can be believed that azote was formed in his experiments, it
must be very evident that he was altogether ignorant of the nature
of the changes that happened in the operations.   Henceforward,
then, azote continued to be considered as a simple body till the
grand discovery of the metallic nature of the fixed alkalies by Sir
H. Davy, who was led by analogy to conclude that ammonia con-
tained oxygen, although it was formed of azote and hydrogen only.
This consideration naturally led to the conclusion that azote con-
tained oxygen in its composition, and its base he conceived to be
hydrogen.   Allen and Pepys about this time being engaged in a
series of experiments on respiration, found a greater quantity of
azote in the products than could be accounted for in the air em-
ployed.   Sir H. Davy again turned his particular attention to the
subject.   As he proceeded in his experiments he found that a certain
quantity of azote had disappeared, while its place was supplied with
a production of oxygen and hydrogen ; he observed, also, when
water was present, a still further increased loss of azote. He hence
concludes,* " that the decomposition and composition of azote
seem proved, allowing the correction of the data, and one of its
elements appears to be oxygen."   From these facts he seemed to
infer that azote was " a compound of hydrogen with a larger pro-
portion of oxygen than exists in water."   Dr. Henry, with a view
to decide this subject, undertook the analysis of ammonia by elec-
tricity ; but, after the most elaborate and careful investigation, he
could not find the least trace of the presence of oxygen. Berthollet
also endeavoured to search for the presence of oxygen in ammonia,
but could detect none.   Sir H. Davy more recently has examined
with great care the results of the action of potassium on ammonia,
and found that in proportion as the olive coloured substance was
free from moisture, there was a less deficiency of azote.   He hence
renounced his idea of the compound nature of azote ; and in his
Elements of Chemical Philosophy he classes it among the simple
bodies.   Sir H. Davy made many direct attempts to decompose
azote itself.   He exposed it to the action of the most powerful
Voltaic pile ever constructed, without the least effect, and heated
potassium in it by the flame of the same apparatus, but no change
was observable.   Its inertness under the influence of the most
powerful Voltaic combinations has been urged as a decisive proof of
the simplicity of this substance : but when we see a compound
formed of four elements resisting all attempts at decomposition,
although it is so easy of formation ; and when we again perceive
that azote and chlorine have refused to unite by means of the most
powerful Voltaic agency, though its combination is now effected by
the most simple means that can be conceived, we need not wonder
that azote should in an equal degree resist its influence.   The evi-

* Phil. Trans. 1809, p. 130.

dence, therefore, of the inaction of azote in the Voltaic circuit is
not fatal to the idea of its compound nature. We may conceive a
body to be easy of decomposition in proportion to the balance of its
opposite electrical forces; but in proportion as a compound pos-
sesses an excess of one kind of energy over another, with so much
more force will the weaker power be held in combination by the
stronger; in such degree will it resist the most violent attempts at
decomposition by the Voltaic pile, and so will it, by this test, the
nearer assume the electro-chemical character of a simple body. It
would be absurd, then, to calculate the simplicity of a body from
its inertness under the Voltaic agency.

Not satisfied with the determinations of Sir H. Davy, and for-
cibly struck with the considerations which I had presumed to oppose
to such authority, I ventured to hazard these views to the trial of
experiment, resolving to operate by methods different from those
hitherto adopted. If azote were a body containing a greater pro-
portion of positive atoms in its composition, it appeared useless to
attempt its separation by exposing it to the action of bodies them-
selves electro-positive; and if it be supposed an oxide of hydrogen,
it seemed possible to produce *azote* from *water* by exposing the
latter to the action of a body possessing a power more strongly
electro-positive than hydrogen; that is to say, one which would
produce a new disposition of electrical forces without resolving
them into simple and separate elements. Sulphureted hydrogen
seemed the body most likely to answer this desired object, as it
possessed so strong an affinity for oxygen under all circumstances,
being composed of bodies both strongly electro-positive. I therefore
determined on the trial of experiment with this test. The first
results answered my fullest expectations. On passing water in the
state of vapour together with sulphureted hydrogen gas through
tubes of porcelain, I found, on repeated trials, azote produced in
abundance.

. In all experiments prior to Jan. 5, 1812, I passed the sulphureted
hydrogen and aqueous vapour through Wedgewood's tubes; but in
order to be certain that the azote was not introduced through the
pores of the porcelain, as was supposed in the experiments of Gir-
tanner, Priestley, and the Dutch chemists, I afterwards made use
of a copper tube without any soldering, accurately fitted into
conical ends of two brass cylinders leading to the other parts of the
apparatus. These conical fittings were perfectly air-tight, but they
were further secured by a luting of borax and pipe-clay. The
copper tube was $1\frac{1}{4}$ inch bore, and 19 inches long. Each of the
brass tubes were 24 inches long, with a diameter of one inch inter-
nally. The one tube had its other end secured by a stop-cock
connected with bladders, or the gazometer containing the recently
prepared gas. In the same tube near this was connected a metallic
vessel by means of a large stop-cock, so contrived that the steam
issuing from the water kept boiling in that vessel might pass into
the room without entering the apparatus, or it might be let into the

tube without any external communication. To the further end of the other brass tube was screwed a glass globe, in which any condensable matter might deposit itself; from this the gases passed onwards through a glass tube into receivers placed on the shelf of the pneumatic cistern. The copper tube was kept at a red heat by crossing a furnace ; and when the water boiled, the cock was turned, so that the aqueous vapour rising into the tubes forced forward the greater portion of air contained in the apparatus into the receivers prepared for its reception. This done, the stop-cock communicating with the gazometer was opened; the sulphureted hydrogen was pressed forward, and meeting with the steam, passed together through the red-hot tube, whence their gaseous products were received in vessels placed on the shelf of the pneumatic trough.

It will not be necessary to describe the whole of the experiments; I shall relate such only of the later ones as appear the most decisive. In an experiment on Nov. 25, 1811, where sulphureted hydrogen (obtained from the solid sulphuret of potash by muriatic acid), and water in the state of vapour, were sent together through a red-hot porcelain tube, the gas that came out did not possess the least smell, nor did it affect the acetate of lead. It contained no hydrogen ; but the eudiometer detected the presence of 20 per cent. of oxygen, and, from its want of chemical proporties, the remaining 80 appeared to be azote. This was made more evident by its physical characters, as the gas possessed the same specific gravity as atmospheric air, and moreover had not the least smell.

In another experiment, on Jan. 5, 1812, sulphureted hydrogen obtained as before, and aqueous vapour, were sent through the copper tube. A quantity of liquid deposited itself inside the glass globe, although in all former experiments no fluid whatever was condensed. The peculiarity here observed may be attributed to a larger quantity of water having passed through the tube, in proportion to the sulphureted hydrogen, and it is probable (from the formula of the result, page 185,) that but a part only of the vapour underwent decomposition. That which passed unaltered condensed with it the sulphurous acid gas in the globe. The fluid, on examination, was found to be water, containing a small portion of sulphurous acid. The gaseous product appeared to be principally azote. It had no smell, and extinguished a burning body when immersed in it. Its specific gravity was 0·978, 100 cubical inches weighing 29·84 grains. It contained no hydrogen ; but it had 3 per cent. oxygen : the 97 remaining parts possessed, in short, all the characteristics that entitled it to be considered as azote.

In another experiment, on Feb. 16 following, I endeavoured to ascertain if water could be decomposed by hydrogen alone. I passed aqueous vapour and hydrogen gas through the copper tube ignited ; the products collected in the pneumatic trough presented only hydrogen gas in the same state as it existed before the experiment.

In two experiments, of March 15 and 22, following, I obtained results as singular as any that had before occurred. This extraordinary difference I conceive to be attributable to the state of the sulphureted hydrogen, which was here obtained from the sulphuret of iron by sulphuric acid : in such cases the gas is generally contaminated with variable proportions of hydrogen. This gas and aqueous vapour were together sent through the ignited copper tube ; the product that came over blackened the inner surface of the glass tube leading to the trough, as well as the inner surface of the receivers in which it was collected. It did not possess the least odour. The gas was found to consist per cent. of 73 volumes of hydrogen and 27 residue. It contained no oxygen ; examined in Hope's eudiometer, sulphuret of potash absorbed about 6 per cent., and a solution of pure potash produced no change of volume in the gas. I endeavoured to ascertain its specific gravity ; but unfortunately did not possess a sufficient quantity to fill the flask, as a quantity of water rose with the gas into the previously exhausted vessel. Making deductions, however, for the weight and measure of the water, as well as of the hydrogen, the residual portion of the gas was found to weigh 8 grains. We hence obtain for the weight of 100 cub. inches 76 grains. This must, however, be considered only as an approximation ; for I am inclined to believe that a trifle too little has been calculated as the weight of the water. The gas possesses all the appearance of sulphureted azote ; but we have never before known such a compound. Concluding it so, and supposing it to be composed of an atom of sulphur and an atom of azote, we calculate for the weight of 100 cubical inches 64·44 grains, an approximation not very distant. I have much to regret that an accident deprived me of the power of making more extended examinations on this gaseous product.

The last experiment that I had an opportunity of making was on March 6, 1812 ; and it will be found to have led to results still more singular, and to consequences more important. Sulphureted hydrogen gas obtained from the sulphuret of iron by dilute sulphuric acid, and vapour from boiling water, were sent, as before, through the copper tube, previously freed as much as possible from atmospheric air. The first portion of gas that came over was removed, in order to be certain that atmospheric air was excluded from the apparatus. The remaining products were set aside for examination. The gas did not possess the least smell. When tried in Hope's eudiometer with hydrosulphuret of potash, four measures per cent. were absorbed, which may be considered as oxygen. Detonated in Volta's eudiometer with oxygen, the mean of two trials indicated the presence of 52 per cent of hydrogen. The residue answered to no test. The specific gravity of the gas I found to be 0·4782 ; 100 cubical inches will hence weigh 14·82 grains. Now the gas was found to consist of

	By Measure.		By Weight.
Oxygen	4	=	1·34
Hydrogen	52	=	1·15
Residue	44	=	12·33
Cub. In.	100	=	14·82 Grains.

And hence we have to determine the nature of this unknown gas. Was it azote? Although it seems to possess the general character of that gas, it is yet too light for it; for if 44 cubic inches weigh 12·33 grains, 100 cubic inches will weigh 28·02 grains; and this is rather lighter than azote. Some considerations towards the conclusion of this paper will make it very probable that it was a compound of sulphur, azote, and hydrogen.

In all these various experiments, although the same methods have been followed, the products have all differed; and it may be inquired why, if the processes were the same, have not similar results been obtained? The cause of this variation will be found, no doubt, to depend partly on the state of the sulphureted hydrogen, of the real nature of which but little comparatively is known. The proportion of water that undergoes decomposition, in relation to the sulphureted hydrogen, must also assist in varying the results. Thus it seems probable that in the experiments of Nov. 25,[*] three atoms of water were decomposed by one of sulphureted hydrogen; in that of Jan. 5,[†] 13 atoms of water by five of gas; in

[*] Supposing the elements of the materials that passed through the process to have been $\frac{3}{W} + \frac{1}{SH}$ or $\frac{9}{W} + \frac{3}{SH}$ or $\frac{9}{O} \frac{9}{H} + \frac{3}{S} \frac{3}{H}$ or $\frac{3}{S} \frac{9}{O} \frac{12}{H}$, then the new arrangements may be thus shown:—

$$
\begin{array}{lll}
3 \text{ S} & = & 3 \\
9 \text{ O} & = & 6 \quad 2 \quad 1 \\
12 \text{ H} & = & \quad\quad 12 \\
\hline
 & & 3 \quad 2 \quad 1 \\
 & & \text{SS} \quad \text{A} \quad \text{O}
\end{array}
$$

The result of this experiment would in such case be three atoms of sulphurous acid (formed of three atoms of sulphur and six of oxygen), two atoms of azote (by the combination of two atoms of oxygen with 12 of hydrogen), and one atom of oxygen, would remain uncombined. The sulphurous acid would be absorbed, and a gaseous product would remain of two atoms of azote and one of oxygen, a proportion that agrees as nearly as possible with 80 azote and 20 oxygen per cent. in volume.

[†] Supposing again the materials passing through the same process to have been $\frac{13}{W} + \frac{5}{SH}$ or $\frac{13}{O} \frac{13}{H} + \frac{5}{S} \frac{5}{H}$ or $\frac{5}{S} \frac{13}{O} \frac{18}{H}$, the new arrangement will be, for

$$
\begin{array}{lll}
5 \text{ S} & = & 5 \\
13 \text{ O} & = & 10 \quad 3 \\
18 \text{ H} & = & \quad\quad 18 \\
\hline
 & & 5 \quad 3 \\
 & & \text{SS} \quad \text{A}
\end{array}
$$

The gaseous product in such cases would be azotic gas only, as the sulphurous acid would combine with the water. These, it will be observed, are exactly the results of this experiment.

that of Feb. 22,* two atoms of water by one of gas; and in that of March 6, (from which was procured the peculiar acid gas described in the latter part of this paper,) one atom of water only was decomposed by one of sulphureted hydrogen.

It is evident from these facts that azote has been formed from the decomposition of water, and that azote is a compound of oxygen and hydrogen, in which the former exists in a much less proportion than in water. Is it possible to establish any data from these experiments that can lead to the knowledge of the proportions in which these elements combine to form azote? In the experiments of Jan. 5, azote was nearly the whole of the gaseous product: of Nov. 25, azote and oxygen uncombined, nearly in the same proportions as they exist in atmospheric air, were the results of the process: of Feb. 22, sulphureted azote and hydrogen were formed from the same elements: and of March 6, hydrogen and a peculiar gas, (a compound of sulphur, azote, and hydrogen,) were the principal products of the new arrangements. In all these changes it must be apparent that by the decomposition of the water the sulphur has become oxidated, and has taken a large quantity of its oxygen to convert it into an acid: it must therefore necessarily follow, that the azote thus produced is a lower oxide of hydrogen than water, and that its ultimate particle contains a greater number of atoms of hydrogen than it does of oxygen. The different electro-chemical habits of water and azote confirm this conclusion: water, because of the equal balance of its electro-chemical forces in the most simple arrangement, can be attracted by neither pole of a Voltaic circuit, but must be separated into its original elements: azote, on the contrary, we know to be strongly electro-positive; and from its being so strongly attracted by the negative pole as to resist the most violent attempts at decomposition, it is a fair conclusion that it contains a preponderating force of positive or hydrogenous atoms. Were it possible to measure the degree of the positive power of azote, we might at once arrive at the proportions of its original elements.

Among different formulæ I have tried, none have been found to agree so well with the results of my experiments as that which

* We will suppose here that the materials passing through the tubes were $\frac{2}{W} + \frac{1}{SH} + \frac{1}{H}$, the sulphureted hydrogen having been obtained from the sulphuret of iron, and hence contaminated by hydrogen. These elements being $\frac{4}{W} + \frac{2}{SH} + \frac{2}{H}$ or $\frac{2}{S} \frac{4}{O} \frac{8}{H}$, the new arrangements will be,

$$
\begin{array}{c|cc}
2\ S & 1 & 1 \\
4\ O & 3 & 1 \\
8\ H & & 6 \quad 2 \\
\hline
 & 1 \quad 1 \quad 2 \\
 & S3O \ SA \ H
\end{array}
$$
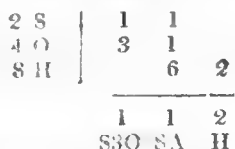
The results of this change will hence be one atom of sulphuric acid, and in the gaseous state one atom of sulphureted azote with two atoms of hydrogen, which will be found to agree exactly with the products of the experiment.

determines azote to be a compound of six atoms of hydrogen and one of oxygen. This proportion, it will be seen, answers exactly in all cases, and it has served as the basis of calculation for the composition of azotic combinations in vol. iii. p. 366, of your Journal.

After I had ascertained the fact that azote was formed from the decomposition of water, I withheld my experiments, for the purpose of procuring a more improved apparatus, that should enable me to ascertain the quantity of azote produced from a given portion of sulphureted hydrogen and water : the data afforded by the previous experiments hardly being of themselves sufficient to deduce with certainty the proportions of the constituents. In the interim unavoidable circumstances arose that prevented the renewal of my labours since that period. Preparing, however, last November, for the renewal of my experiments, I was induced to take up the flask containing a portion of the gaseous products of the last experiment of March 6, 1812. On opening the stop-cock, I was surprised to find a sudden gush of air into the flask, amounting in quantity, I conceived, from the time it took, to near a third of its contents. I was yet, however, more astonished, on discovering that the gas had now a smell similar to that of sulphureted hydrogen ; if any thing, more unpleasantly fetid. Much struck with the singularity of these circumstances, I was induced to examine the flask, when I perceived the internal surface of the glass to be covered with innumerable transparent brilliant white crystals, shooting generally from an arborescent efflorescence that had deposited itself over the whole internal superficies, and a small deposition of moisture. These crystals I presume, from the well-formed regularity of their shape, to have been arranging themselves long before that period. I can recollect that, above a year before, I opened for an instant the stop-cock of the flask, covering the aperture with my finger, when a small absorption of air took place, which I then attributed to the change of volume from difference of température. Last summer I recollect perceiving the whitish appearance of the flask ; but not suspecting the occurrence of so singular a change, the circumstance was passed over without further observation. I was now led to weigh the flask, in order to ascertain the quantity of air that had admitted itself. The contents of the flask on March 6, 1812, weighed 5·5 grains : on Nov. 28, 1813, it was found to have increased in weight 7·25 grains. My next object was the extraction of the gas from the vessel for examination, and then to ascertain the amount of the solid materials deposited from the gas. On the exhaustion of the flask its gaseous contents were found to amount in weight to 10·75 grains. There remains, therefore, for the weight of the materials deposited, two grains.

Description of the Crystals.

The crystals, though small, are of very regular formation, being very thin plates of a rhomboidal shape : sometimes we meet with

those whose acute angles are truncated. When examined by a lens, a facet is seen on the smaller edge of the rhomb, forming with its flat face an angle of about 10°. I endeavoured to ascertain the angles of the crystals by Wollaston's goniometer; but the plates were so minutely thin, that no light could be seen reflected on the outer edges. As far as my eye enabled me to judge, on comparing them with different lines, the angles of the rhomboidal plates are about 126° and 54°, and those of the six-sided laminæ about 126°, 143°, and 91°. The length of the largest crystals never exceeds $\frac{1}{20}$th of an inch; the surface is very brilliant; they are perfectly transparent and colourless; but when viewed by transmitted light, appear of various prismatic colours. Exposed to heat, they melt and dissipate in air, a smell of sulphurous acid being exhaled.

Examination of the Gaseous Product.

As much of the gas as could be collected was received in vessels in the mercurial trough; and it is to be observed, that all the experiments hereafter detailed were performed over mercury. Knowing that a considerable quantity of oxygen was present in the gas, my first endeavours were to detect its amount; but to my great astonishment, not the least trace of its presence was discoverable by any of the usual tests. With nitrous gas no red fumes were produced, but a very slight whitish cloudiness, faintly perceptible, appeared, which afterward condensed on the sides of the vessel. The mixture was reduced in volume when equal portions were combined, the diminution amounting to 11·94 per cent. of the whole. Detonated in Volta's eudiometer by itself and with hydrogen, at repeated intervals, no explosion would ensue. Examined in Hope's eudiometer with a solution of the sulphureted hydrosulphuret of potash, an absorption of about 40 per cent. took place; but this was evidently condensed by the water of the solution, as water itself was found to absorb the same quantity.

The gas was now examined for hydrogen. In Volta's eudiometer with hydrogen, no explosion was produced on sending several shocks through it. Allowing the mixture to stand for a day, and then repeating the discharge, an explosion immediately took place; the amount of whose diminution indicated the presence of 42·42 per cent. of hydrogen in the gas. In the residue, after its absorption by water, there is found a portion of hydrogen amounting only to 18·61 per cent. in the gas employed: and in the residuum, after its action on the metallic salts, hydrogen is found in variable quantities, amounting on an average to about 36 per cent. in volume of the original gas. The cause of this variation in the appearance of hydrogen in the gas will be accounted for in the sequel. The gas reddens infusion of litmus, and a portion of it combines with alkalies, earths, and metallic oxides. This portion, therefore, possesses most decided characters, while all the residuum appears to be azote only. The gas, then, is seen to consist of an acid gas; a quantity of hydrogen gas that appears intimately combined with

it, and to be always disengaged from it whenever it enters into combination; and a residuum of azotic gas. Its action on the various re-agents must be attributed entirely to the agency of the peculiar portion, and the chemical effects of this mixture will therefore be enumerated as the properties of the acid gas. It will afterwards be my object to develope from these experiments the nature of the changes that have taken place throughout the whole series of operations. I shall designate the air extracted from the flask for examination by the name of the *mixed gas*; and the portion to which it owes its peculiar properties, the *acid gas*.

Properties of the Acid Gas.

The specific gravity of the mixed gas is 0·9348, 100 cubic inches weigh 28·5 grains; but it will afterwards be shown that the specific gravity of the acid gas is 1·16, and that 100 cubic inches weigh 35·38 grains. Whether it has the smell of the mixed gas, or whether that odour is owing to its combination with hydrogen (as is the case with sulphureted hydrogen), I am not able to determine. The hydrogenated acid gas is inflammable, burning with a blue lambent flame, and explodes when previously mixed with a certain portion of atmospheric air. The acid gas is colourless and permanently gaseous over mercury, but it tarnishes rapidly the surface of the metal; and when kept over it a long time, the gas diminishes slowly in volume, a black substance floating on the surface, and adhering to the tube, which is a combination of the mercury with the acid gas.

It is absorbed by water most rapidly; on passing the mixed gas through a column of water once or twice, nearly the whole of the absorbable portion is condensed, the amount of the entire diminution being 39 per cent. Although this quantity only would appear to be absorbed by water, yet it will afterwards be shown that in reality about 50 per cent. are dissolved; the hydrogen which existed in a condensed state in combination with the acid gas is liberated, and hence assumes a larger volume than it occupied when in a state of combination: the actual absorption is therefore in appearance diminished by the excess of volume which the hydrogen occupies in its natural state over that it possesses when in the condensed state of combination with the acid gas. The acid gas is itself wholly soluble in water, which takes up twice its volume. The aqueous solution has the property of precipitating metals from their solutions in the same state as is produced by the acid gas. It has the peculiar smell of the gas, and has a fetid saline taste.

0·22 cubic inches of the mixed gas were passed up into Volta's eudiometer over mercury, through which several electrical discharges were sent without any effect: 0·23 cubical inches of atmospheric air were then added to it; but still the shock produced no effect. Twelve hours afterwards I observed a pretty copious deposition of a yellow substance (most probably a portion of its sulphur) on the inside of the tube, but the volume of the gases remained un-

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altered. Twelve hours after this I again repeated the electrical discharge, when an explosion was in consequence produced, which caused the diminution of the gases to 0·31 cubic inches. This diminution indicates of hydrogen in the proportion of 42·42 per cent. of the *mixed gas* first employed.

A portion of nitrous gas was mixed with an equal measure of *mixed gas*, in order to detect the presence of oxygen. A slight white cloudiness appeared, which deposited itself on the sides of the vessel. A diminution of volume ensued, amounting to about 11·94 per cent. of the whole mixture. This mixture was examined about a fortnight after ; it then retained the smell of sulphureted hydrogen, but none of nitrous gas. An electrical discharge sent through it produced no change of volume. An equal portion of atmospheric air was now mixed with it ; but no change of colour, nor any diminution of volume, was observable. Electrical shocks still refused to produce any effects. These combined gases afterwards acquired the smell of ammonia.

Phosphorus heated in the *mixed gas* rose in a state of white fume, which condensed in a white crust on the surface of the mercury : there was, however, no luminous appearance. The gas, on cooling, was found diminished in volume 11·11 per cent.

A portion of the phosphorus was condensed on the sides of the retort, and a black crust formed on the sides of the tube near the surface of the mercury. The residual gas no longer acted upon mercury, though standing over it several days ; it had also lost its offensive smell. This residuum, when exploded in Volta's eudiometer with atmospheric air, indicated the presence of 30·98 per cent. of hydrogen in the mixed gas originally employed. The tube in which the phosphorus had been sublimed in the gas was found to emit copious white fumes, although the temperature was only 35° Fahr. On applying heat to the black crust, white fumes arose : it disappeared, leaving small globules of running mercury. The gas remaining after the explosion was passed through solutions of acetates of zinc, lead, and barytes ; but no changes were produced. A portion of the residual gas, after the action of phosphorus on the *mixed gas*, was sent through a solution of acetate of zinc, when a very slight white precipitate collected, and the gas diminished in volume 40 per cent., a quantity equal to 35·6 per cent. of the original mixed gas. There has been then on the whole a diminution of 35·6 + 11·11 = 46·7 per cent. in the mixed gas originally employed. Another portion of this same residuum was passed through a solution of acetate of lead, on which a dark precipitate immediately formed ; but an accident prevented an observation of the result. The remaining portion of this residuum was fired in Volta's eudiometer, without any effect ; but when mixed with atmospheric air, it exploded ; and the diminution that followed indicated the presence of a quantity of hydrogen equal to 30·98 per cent. of the mixed gas originally employed.

The acid gas resembles most in its chemical relations sulphureted

hydrogen; but in its more essential properties it will be seen to be distinctly different from it.

It combines with potash, and forms with it a compound insoluble in water. This salt, if it may be so called, is of a dead black colour, and is not acted on by acids or alkalies. Among the many trials I have made, not one acid has yet been found to possess so strong an affinity for the different bases as this singular gas.

It combines also with soda, forming a velvety black compound, equally insoluble in water, and undecomposable by acids or alkalies.

It unites also to pure barytes, and forms a very soluble salt. This solution is decomposed by the acetate of lead, which throws down a dark precipitate, consisting of the acid gas combined with the oxide of lead. The barytes salt is also decomposed by carbonate of potash, a precipitate is thrown down of a grey colour, doubtless a mixture of the potash salt with the carbonate of barytes. This would prove that the acid gas exerts a nearer affinity to the acid gas than barytes does. The results of these experiments are seen in the subsequent table.

The action of ammonia on the acid gas presents the most singular anomaly, and deserves a more particular mention. The action of the fixed alkalies and metallic solutions invariably have diminished the volume of the mixed gas; but liquid pure ammonia has a contrary effect, and expands its volume, 100 measures of mixed gas being enlarged to 121 measures. This residuum, it will appear strange, contains no hydrogen that is discoverable by the usual tests. The solution acquired a greenish tinge, and a very slight green precipitate collected. This fluid and precipitate, by exposure to air, acquired an orange-yellow colour. The following are the particulars of the experiment. Into a tube containing 0·175 cubic inches of gas over mercury was sent up 0·11 cubic inches solution of pure ammonia; a white smoke appeared to pervade the gas, and the fluid became tinged of a brownish green colour. The gas, however, expanded in volume to 0·215 cubical inches, indicating an enlargement of 22·85 per cent. Two other experiments produced similar results. The mean increase of volume in the three trials was 22 per cent. The residue was not inflammable, nor would it detonate with oxygen. Four days afterwards I transferred 0·1 cubic inch of this residuum into Volta's eudiometer with 0·11 cubic inches of atmospheric air over mercury, into which a portion of the solution and precipitate had unavoidably introduced itself. No explosion or variation of volume was produced by the electrical discharge; but some hours afterwards I perceived the fluid, before of a dark green colour, was converted into an orange yellow, and the inside of the tube was also covered with an orange coloured coat. Firing afterwards several shocks through it, no effect was to be produced.

The acid gas displaces the carbonic acid from its combinations; with bicarbonate of potash the insoluble combination of acid gas and potash is precipitated, and with ease.

The acid gas appears, in short, to possess very strong affinities, as it displaces most other acids from their combinations, and is not itself to be separated from alkalies and metallic oxides when once united with them.

It will be better to present under a tabular form the general results of the action of different re-agents on the mixed gas, as it will occupy too much space to enumerate the particulars of each experiment.

Re-agents.	Effects.	Change of volume of 100 measures to	Hydrogen in the residuum per cent.	Hydrogen in mixed gas employed per cent.
Atmospheric air	{ After long standing, exploded by electricity....			42·42
Chlorine gas..	{ Immediate diminution of volume to .... The residuum contains 26·66 per cent. that precipitates acetate of lead white.	84·76	None	
Nitrous gas...	White fumes	88·06	None	
Water	{ Absorbs the whole of the acid gas	61	45·18	27·56
Phosphorus ..	{ White fumes, when heated, diminishes to	88·88	36·11	30·98
Potash	{ Brownish black precipitate, insoluble .... Ditto	63·88 68·83	52·94	36·44
Soda........	{ Velvety black precipitate, insoluble	75·75	38·56	29·21
Ammonia....	{ Green precipitate, expansion of volume to	121	None	
Lime	Soluble combination......	69·25		
Barytes	Soluble compound	62·72		
Bicarbonate of potash..	{ Displaces carbonic acid, and forms a brownish solution	None		

In the action of the mixed gas on metallic solutions some curious effects will be observable, such as will sufficiently distinguish it from sulphureted hydrogen. The latter, it is well known, does not precipitate iron and nickel from their solutions ; but this acid gas will be found to form insoluble compounds with those metals. The appearance and colours of the precipitates differ in most cases from those produced by sulphureted hydrogen. The amount of apparent absorption will be seen to vary in the different solutions, but these anomalies will be made to disappear in the sequel. The great distance of time at which many of them were made has in some small degree influenced the results. The gaseous residues of all the different trials were inflammable ; and I have to regret that want of time prevented me from having ascertained the quantity of hydrogen in each.

Metallic Solutions.	Colour of the Precipitate.	100 measures of gas reduced to	Quantity of hydrogen in residue.	Do. apparently in the mixed gas employed.
Muriate of platina	Light brown.............	52·2		
Muriate of gold	Dark brown.............	64·71		
Nitrate of silver	Black	61·82		
Nitrate of mercury....	Yellow	64·29		
Oxymuriate of ditto ..	Grey black..:..........	63·5		
Prussiate of ditto	Dark brown	46·		
Ditto, ditto	Ditto	43·13	30·303	13·071
Sulphate of copper....	Black	62·13		
Acetate of iron	Light brown	65·22	45·75	29·89
Muriate of tin	Dark brown	66·98		
Acetate of lead	Black scales...........	62·		
Ditto, ditto	Ditto	61·37		
Nitrate of nickel......	Copious thick black	58·67		
Acetate of zinc	White................ ...	66·6		
Muriate of bismuth ...	Red brown scales	66·22	51·11	30·46
Muriate of antimony ..	Ash	60		
Acid of arsenic........	Yellowish green solution.	66·19		
Acetate of cobalt......	Reddish brown..........	66·29		

(*To be continued.*)

Article V.

Description and Analysis of the Mineral called Hauyne. By
Leopold Gmelin, M.D.[*]

THIS mineral has been hitherto found only in three places, all of
them considered as volcanic; namely, the neighbourhood of Rome,
Naples, Andernach, and in Auvergne. It was first observed near
Rome by Gismondi, who gave it the name of *latialite*. Neergaard
afterwards described it, and gave it the name of Hauyne in honour
of the Abbé Haüy, to whom mineralogy lies under so many obli-
gations. Haüy has given a short account of it, with an imperfect
analysis by Vauquelin, in his Tableau Comparatif. This analysis
was as follows :—

Silica .... 30·0
Alumina .... 15·0
Sulphate of lime .... 20·5
Lime............................. 5·0
Potash............................ 11·0
Oxide of iron .... 1·0
Sulphureted hydrogen .... ⎱ 17·5
Loss .... ⎰

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100·0

[*] Abstracted from his thesis printed at Heidelberg in 1814.

Gmelin found it most abundant in the lava called *peperino*, below the town called Marino, in the neighbourhood of Rome. It occurs in a rock along with green mica, augite, and a white coloured mineral, which from the analysis of Gmelin appears to be a variety of *leucite*.

The colour of *hauyne* is Prussian blue, passing into beryl blue, through sky blue.

Gmelin found only one specimen crystallized in an imperfect octahedron. It is almost always imbedded in the rocks in the state of grains with sharp angles. In some rare cases only can a single cleavage be observed in it.

Its fracture is conchoidal, and it breaks into fragments having sharp edges.

Lustre, splendent and vitreous.

It is always translucent.

Its hardness is such that it easily scratches glass ; but does not strike fire with steel.

It is easily frangible, and may be reduced with facility to powder. Powder whitish.

Its specific gravity, according to Gismondi, is 3·333 ; according to Neergaard, 3·100 ; according to Gmelin, 2·833.

It does not phosphoresce when scratched with a knife. It becomes electric by friction, according to Haüy, and the electricity is negative.

Before the blow-pipe it melts with difficulty into an opake bead, full of air bubbles. With borax it melts into a transparent topaz coloured bead. When put into muriatic acid it becomes opake, and loses its colour. When thrown into that acid in the state of powder it emits the odour of sulphureted hydrogen gas, and gelatinizes.

To subject it to a chemical analysis it was necessary to obtain it separate from the other minerals with which it is usually mixed. This was a task attended with considerable difficulty, and took Gmelin up a full week before he could procure a sufficient quantity of the pure mineral for his experimental investigation. The following was the way in which his chemical analysis was conducted :

A.

a.—0·47625 gramme (7·355 grains troy) of hauyne in grains were put into a glass tube, and surrounded with live coals. Vapour of water was immediately given out, which did not alter the colour of paper dipped into acetate of lead. The tube being heated till some of the grains adhered to it, the weight of the hauyne was found reduced to 0·473 gramme (7·305 grains) ; but its colour and transparency were not altered.

b.—The same mineral being exposed for half an hour to a strong fire in a platinum crucible, the grains adhered a little to each other, and to the crucible. Many of the particles were not altered, but some acquired a blackish blue colour, and at the same time lost their translucence. By this means the weight was reduced to

0·4705 gramme (7·266 grains). Hence the whole diminution amounted to 0·089 grain, or to 1·20 per cent. This loss must be ascribed to water driven off by the heat.

But as hauyne, when fused before the blow-pipe, forms a bead filled with bubbles, it is obvious that it must contain a greater quantity of water than this.

B.

a.—A quantity of hauyne of a slight greenish tint, to which only a little mica adhered, was reduced in an agate mortar to a very fine powder, which had a greenish white colour.

b.—Of this powder 4·771 grammes (73·683 grains) were thrown gradually into muriatic acid a little diluted. Immediately the odour of sulphureted hydrogen gas was emitted, and paper dipped in acetate of lead was stained black : yet no bubbles of this gas could be seen separating from the powder. The first portions of hauyne were slowly dissolved, giving the liquid a greenish colour, and rendering it somewhat turbid; but the last portions were scarcely attacked. By the assistance of heat, however, the whole was gradually and equally acted upon, and the liquid reduced to a jelly of a greenish yellow colour ; which, however, was not transparent, as Vauquelin says he observed it to be.

This jelly, being gradually inspissated by a gentle heat, was mixed with water, and then gradually evaporated to dryness. It constituted a lemon yellow mass, which, when quite dry, assumed a dirty white colour. This matter was again mixed with dilute muriatic acid, boiled for a considerable time, new acid being constantly added, and then reduced again to a dry powder.

c.—This powder was boiled in water acidulated with muriatic acid for a quarter of an hour, and then the whole was passed through a filter. The undissolved powder, being thus collected on the filter, exhibited no traces of containing any of the mineral unacted upon by the acid.

d.—This powder, dried on the sand-bath, weighed 2·077 grammes (32·077 grains). Of this 1·688 gramme, being exposed to a strong red heat for half an hour, was reduced to 1·502 gramme. Hence the whole mass, if thus ignited, would have been reduced to 1·848 gramme (28·54 grains).

e.—As the powder, when exposed to a red heat, split into prisms, and as the quantity of silica thus found far surpassed what Vauquelin had found in hauyne, I suspected that, notwithstanding the long continued action of the muriatic acid, a portion of alumina might still adhere to this powder, and of course that it was not pure silica. This suspicion was confirmed by boiling in muriatic acid the portion of the powder that had not been ignited. Alkalies, either caustic or carbonated, being dropped into this acid, threw down a white powder, while muriate of barytes occasioned no precipitate.

f.—Therefore I fused the whole of the ignited powder in a platinum crucible with caustic alkali, dissolved the fused mass by

means of water and muriatic acid, and then evaporated the solution to dryness. From this dry mass the lime and alumina were dissolved by means of muriatic acid boiled upon the powder. The acid solution being mixed with a great excess of caustic potash, the lime was thrown down and separated by the filter. The alumina was then separated by means of sal ammoniac, and likewise collected on the filter. By dividing the loss which in working upon such small quantities is unavoidable, we may estimate the 1·848 gramme of ignited powder to consist of 1·6905 gramme (26·108 grains) of silica, 0·0825 gramme (0·1397 grain) of alumina, and 0·075 gramme (0·1158 grain) of lime.

It is obvious from this that hauyne cannot be accurately analysed by means of muriatic acid; as it is always to be feared that the silica separated by its means retains a portion of the other earths, and even of the potash.

g.—The liquid separated from the powder in paragraph *c* was limpid. It was evaporated in a glass capsule till it deposited needle-form crystals, and assumed a yellow colour. It was then gradually reduced into a yellow pulverulent mass, which rapidly absorbed moisture from the atmosphere.

h.—This dry mass was repeatedly boiled in different portions of the pure alcohol of Richter, which were successively decanted off and poured upon the filter.

i.—The powder remaining upon the filter, being edulcorated with a sufficient quantity of hot alcohol, was white; and being dried upon the sand-bath, weighed 2·330 grammes (35·985 grains). Of this 1·618 gramme, being ignited for half an hour, melted into a dirty white mass, which was a little swelled, and weighed 1·496 gramme. As this diminution of the weight appeared too small on the supposition that the greatest part of the mass consisted of sulphate of lime, it was exposed for half an hour longer to a still stronger heat, by which the weight was reduced to 1·336 gramme. It had been again melted, and the colour was become more dirty than before. As this mass, as we shall see immediately, contained a good deal of muriate of lime, there is reason to suspect that a portion of this salt was volatilized by this repeated exposure to so high a temperature.

k.—Water was boiled on the portion of the powder of paragraph *i* that had not been exposed to a red heat. This liquid, after filtration, precipitated in abundance sulphate of silver, and was not rendered turbid by caustic ammonia. The carbonate of ammonia precipitated white flocks from the hot solution. The fluid separated from these being evaporated, formed a granular precipitate, with a solution of platinum.

l.—The presence of muriate of potash in the powder being thus recognised, the ignited portion of the powder (*i*), weighing 1·336 grammes, was treated with a sufficient quantity of water. This liquid being filtered and evaporated, yielded at first needle-form crystals; which being edulcorated with some water, and afterwards

with some alcohol; the liquid set to evaporate in the sun deposited large cubes, which being fused in a platinum crucible weighed 0·654 gramme.

The portion of sulphate of lime which had precipitated weighed about 0·01 gramme. Muriate of barytes and oxalate of potash formed nearly the same quantity of precipitate when dropped into the solution. The oxalate of lime thus obtained being dissolved in muriatic acid, the sulphuric acid was precipitated from the liquid by means of muriate of barytes. The sulphate of barytes obtained weighed 0·049 gramme, indicating, according to the estimate of Berzelius, 0·01666 gramme of sulphuric acid. Hence we may estimate the sulphate of lime, on the authority of Bucholz, at 0·0286 gramme. This quantity, together with the 0·01 gramme of sulphate of lime not dissolved, being subtracted from 0·654 gramme of salt, leaves 0·616 gramme for the weight of the whole muriate of potash contained in the ignited powder.

Now according to Bucholz, 0·616 gramme of muriate of potash contain 0·498 gramme of potash. Hence the whole mass, amounting to 2·330 grammes, must have contained 0·7172 gramme of pure potash.

m.—The sulphate of lime (in paragraph *l*) left undissolved being collected from the three filters, weighed after ignition 0·527 gramme, and was a white powder. Now as the gypseous mass heated to redness weighed 1·336 gramme, from which 0·616 gramme of muriate of potash, and 0·038 + 0·527 gramme of sulphate of lime, were obtained, it follows that there is a deficit of 0·155 gramme, which, as the filters were always well washed, must be ascribed to sulphate of lime adhering to these three filters; so that the quantity of sulphate of lime must amount to 0·720 gramme, or to 1·037 gramme in the whole gypseous mass.

n.—The alcohol solution of paragraph *h*, being mixed with water, was evaporated, some drops of nitric acid being added during the evaporation to oxidate the iron to a maximum, and gradually reduced to a dry mass. The mass, being mixed with water, was passed through the filter, in order to separate some floccules of silica, which might be estimated at about 0·002 gramme. A sufficient quantity of caustic ammonia being added to the filtered liquid, a copious white precipitate fell, which was separated by the filter.

o.—This precipitate, being separated from the filter, was boiled, while still moist, in caustic alkali. The whole was dissolved, except some flocks of hydrate of iron, which being collected on the filter, washed, and dried on the sand-bath, weighed 0·075 gramme, indicating about 0·06 gramme of oxide of iron.

p.—The alkaline solution of the last paragraph, being mixed with sal ammoniac, let fall a white matter, which, when edulcorated, and dried on the sand-bath, weighed 1·120 gramme. Of this 1·058 gramme, being heated to redness in a platinum crucible for half an hour, was reduced to 0·697 gramme. Hence the

weight of the whole alumina would have amounted to 0·737 gramme. To this must be added what adhered to the filter in paragraph *o*, from which the matter was scraped off to be boiled in the alkaline ley. Its weight was increased 0·124 gramme. If we consider this as a hydrate of alumina, it will be equivalent to 0·081 gramme of ignited alumina ; so that the weight of the whole alumina separated from the silica by means of muriatic acid is equivalent to 0·818 gramme.

q.—The liquid (paragraph *n*) freed from its alumina and iron by means of caustic ammonia, being filtered and evaporated, was mixed boiling hot with carbonate of ammonia. It deposited a quantity of carbonate of lime, which, being washed, and dried on the sand-bath, weighed 0·092 gramme ; indicating, according to Berzelius, 0·0519 gramme of pure lime.

r.—The liquid thus freed from lime, being evaporated to dryness, and heated in a platinum crucible, to separate the sal ammoniac, left 0·03 gramme of muriate of potash, which, being dissolved in water, formed a granular precipitate with the solution of platinum, and yielded cream of tartar when treated with tartaric acid. This 0·030 gramme of muriate of potash contains, according to the estimate of Bucholz, 0·02 gramme of pure potash.

The preceding analysis gives us the constituents of hauyne as follows :—

Water			1·20
Silica	$\begin{cases} b \ 1\cdot6905 \\ n \ 0\cdot0020 \end{cases}$	35·48
Alumina	$\begin{cases} b \ 0\cdot0825 \\ p \ 0\cdot8180 \end{cases}$	18·87
Sulphate of lime	m 1·0368	21·73
Lime	$\begin{cases} b \ 0\cdot0750 \\ q \ 0\cdot0519 \end{cases}$	2·66
Oxide of iron	o 0·6	1·16
Potash	$\begin{cases} n \ 0\cdot7172 \\ r \ 0\cdot0200 \end{cases}$	15·45
Sulphureted hydrogen and loss		3·45
		100·00

As my method of analysing hauyne was the same as that of Vauquelin, it may be worth while, in order to account for the great loss which is exhibited in his table, to consider the nature of the constituents of this mineral with some attention.

It is obvious, in the first place, that the sulphate of lime cannot be completely separated from the other salts by means of alcohol. Hence it would have been better to have determined the quantity of sulphuric acid in the first place by means of muriate of barytes, and then having removed the excess of barytes, to have precipitated the whole of the lime in the state of carbonate.

As hauyne is not completely decomposed by muriatic acid, the silica still retaining a portion of alumina, and even of lime, after having been repeatedly boiled in that acid, it does not appear that a very accurate analysis can be expected when conducted by means of that acid. It would have been better to have fused the hauyne with potash, and with nitrate of barytes, in order to obtain its several constituents in a state of purity. But as I had not a proper quantity of hauyne in a state of sufficient purity, I was under the necessity of deferring that mode of analysis to another time.

The traces of copper suspected by Vauquelin I could not detect by means of sulphureted hydrogen gas.

The great quantity of sulphuric acid in this mineral, while its hardness is such as to scratch glass, is surprising. This circumstance distinguishes hauyne from all other minerals. Probably this acid in the mineral is not merely in combination with the lime, but with all the constituents, and particularly with the potash, from which it is separated by the action of the muriatic acid.

The emission of sulphureted hydrogen gas when hauyne is treated with muriatic acid is no less remarkable. No traces of pyrites, or any similar mixture, can be perceived in it. Hence this gas would seem to be chemically united with the other constituents of the mineral.

The presence of sulphur in it seems to be connected with the geognostic relations of hauyne; for as it is always found in the neighbourhood of beds producing volcanic fire, and therefore abounding with sulphur, we may conclude that it is of contemporaneous formation with these beds.

I consider hauyne as nearly related to the family of zeolites.[*] It agrees with them in its specific gravity, hardness, and other external characters; it gelatinizes in acids, melts before the blow-pipe with effervescence, and contains a quantity of potash and the same kind of earths that the zeolites contain. It differs from them in the great quantity of sulphuric acid which it contains. On this account it would seem to be related to the lapis lazuli; though it is probable that the sulphate of lime found in that mineral by Klaproth was owing to the presence of some foreign body with which it was contaminated, as no such substance was found by Clement and Desormes in ultramarine; but both lapis lazuli and hauyne, if we except the colour, agree in several properties which belong to the zeolites.

* I speak of the family of zeolites as established by Haussman in his Handbach der Mineralogic, b. ii. p. 547.

Article VI.

*Memoir on the Sea Water on the Coasts of France, considered in a
chemical and medical point of view.* By MM. Bouillon La-
grange, and Vogel.*

We shall not attempt here to notice the phenomena which the
sea presents. The most astonishing is, no doubt, the ebbing and
flowing of the tide. This is a daily, periodical, and regular mo-
tion, observed in the water, to which the name of *tide* has been
given. On this subject interesting details may be seen in the
researches of Euler, Daniel Bernoulli, and Maclaurin.

The luminous appearance of the surface of the sea has likewise
excited the attention of navigators and philosophers. Some have
ascribed it to a phosphorescent matter, while others have considered
it as an electrical phenomenon.

As to the saltness of the sea in general, it is very difficult to
account for it. Some philosophers have supposed that the sea is
perpetually dissolving the salt which is accumulated at its bottom ;
and that its saltness has existed from the first, because it contains
animals which cannot live in fresh water. Be that as it may, the
sea is more salt in hot regions than in the temperate parts of the
earth ; perhaps on account of the greater evaporation which takes
place in these parts. Sea water has likewise a bituminous taste,
often injurious to weak stomachs.

Among the attempts made for many years to deprive sea water of
its salt, those of Mr. Appleby have had a kind of celebrity. They
were the consequence of the ideas of Hales, who gave on this sub-
ject very interesting details. His method consisted in putting four
ounces of solid caustic potash, and as much calcined bones, into
about 80 pints of sea water. The water distilled from this mixture
becomes sweet ; but still retains a disagreeable taste. Rouelle
repeated these experiments with success. M. Poissonier, sen. a
Member of the Faculty of Medicine of Paris, likewise made expe-
riments on this subject. His method was distillation in an appa-
ratus consisting of tin tubes placed at the orifice of the alembic.
He added some alkali to sea water before distilling it.

Another method was proposed, in 1772, by Dr. Irving. It con-
sisted in a single large tube in place of the worm of the still,
without capital, and without mixing any ingredient whatever with
the sea water. This tube, which has the form of a funnel, is
capable by its shape of being applied to the mouth of all the vessels
used on ship-board for boiling.

MM. Macquer and Monnet likewise proved that sea water might
be distilled without any mixture, and a potable liquid obtained.

* Translated from the Ann. de Chim. vol. lxxxvii. p. 190.

Nature presents us with a method in winter of separating the salt from sea water; namely, by freezing it. It is affirmed that the brewers in Amsterdam employ sea water thus frozen, instead of fresh water, in the manufacture of beer. We have seen pieces of ice from sea water perfectly fresh.

M. Rochon, Member of the Institute, has just pointed out a method of procuring sea water free from any empyreumatic taste by distilling it in a vacuum.* The machine described by that philosopher is extremely simple. All artists may easily execute it without a model.

The analysis of the water of different seas has been made by different chemists.

Bergman, from a kanne (189·94 cubic inches English) of sea water taken up at the Canaries, obtained

> Muriate of soda .... 2 ounces 433 grains
> Muriate of magnesia .... ˙38
> Selenite 45

Forty pounds of water from the English Channel, taken up at Dieppe, and analysed by Lavoisier, gave for result—

<div align="right">Grains.</div>

> Lime and sulphate of lime 128
> Common salt 5072
> Glauber salt and Epsom salt 288
> Muriate of magnesia 576
> Muriate of lime and magnesia 946
> ———
> 7010

The water of the Baltic was analysed at different epochs by three chemists. The result of these analyses varies remarkably ; though the water had been always taken up nearly in the same latitude. The water analysed by M. Link was taken up at Doberan, some leagues from Rostock.†

M. Pfaff took up the water which he examined at Travemunde, near Lubeck. This philosopher found in it so great a proportion of muriate of lime, that there is reason to suspect some mistake in his analysis.

The last analysis of the waters of the Baltic was published by Mr. Luhtenberg, of Dantzick. The water examined was taken up some leagues from the mouth of the Vistula. The results of this analysis approach our own analysis of the water of the English

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* Journal de Physique, lxxvi. 373.

† There are sea water baths in this place. This excellent establishment attracts every year a great number of invalids, who derive very great advantage from these baths. This will be evident to every one who consults the Annals of Doberan, edited by Professor Vogel, Physician to the Duke of Mecklenburg, and Director of the Baths.

Channel; but they differ prodigiously from those of M. Pfaff, as may be seen in the second volume of Schweigger's Journal.

The most remarkable analysis, as far as the results are concerned, is that of the water of the Dead Sea by Klaproth.* This water had been likewise analysed by Macquer, Lavoisier, and Sage. According to Klaproth, its specific gravity is 1·245; and 100 parts of it yield the following constituents :—

Muriate of magnesia	24·20
Muriate of lime	10·60
Muriate of soda	7·80
	42·60†

The specific gravity of sea water, at a mean temperature, may be fixed at 1·0289.

Although the three seas, the waters of which we have analysed, communicate with each other, we have supposed a difference between them in consequence of the difference of latitude; and we have been tempted to believe that if these waters do not differ in the nature of the salts which they contain, they may nevertheless differ in the quantity of saline matter which they respectively contain.

The water which we employed was taken from the surface of the sea,‡ on a calm day, and always at the same season of the year.

The water of the Mediterranean was sent us by M. Poutet, apothecary at Marseilles. It was taken up at the distance of some leagues from that place. For the water of the Atlantic Ocean we are indebted to the complaisance of M. Lebœuf, apothecary at Bayonne. The specimen was taken from the gulf of Gascony. The water of the English Channel was taken up at Havre, seven leagues from the shore, by M. l'Abbé des Fontaines, and was sent us by M. le Bret, apothecary at Rouen. M. Houest, Mayor of Dieppe, had likewise the complaisance to send us water from the English Channel, taken up two leagues to the north of Dieppe, from the middle of the sea.

All these specimens of sea water were first examined by means of re-actives. The results were as follows :—

1. With oxalate of ammonia, a scanty light precipitate.
2. Carbonate of potash and of soda throw down a precipitate.

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* The lake Asphaltes in Judea, known by the name of the Dead Sea, which in consequence of an earthquake inundated Sodom and Gomorrah, as the Scripture informs us, is remarkable for the great quantity of salt which it contains. The bitter saline substances which it contains prevent animals and vegetables from living in it: hence the name Dead Sea. Its specific gravity is so high that it can support weights which would sink in fresh water.

† See for this analysis *Annals of Philosophy*, No. I. I consider the analysis of Dr. Marcet and Mr. Tennant as more precise. See *Annals of Philosophy*, No. II.—T.

‡ The water taken from a great depth at the Canaries, and analysed by Bergman, contains more salt than water taken from the surface of the sea.

3. Ammonia occasions a very copious precipitate; and if we add an excess of ammonia and filter, the filtered liquid, when mixed with potash, lets fall a white precipitate. The precipitate obtained by potash is entirely soluble in sulphuric acid.

4. Muriate of barytes and acetate of lead occasion copious precipitates, which are insoluble in nitric acid.

5. Though it be extremely probable that sea water contains a little of the animal matter called by medical men *bituminous matter*, yet neither the tincture of nutgalls nor chlorine produced any sensible change when mixed with 122 cubic inches of this water.

6. Bicarbonate of potash occasions no precipitate in sea water; but if the mixture be boiled, a white powder is deposited, which dissolves with effervescence in sulphuric acid.

7. The colour of syrup of violets is not sensibly altered by sea water. A great quantity of this water renders tincture of turnsol red; but it recovers its blue colour when the liquid is boiled.

Sea water renders the syrup of buckthorn green. This change may be ascribed to the action of the earthy salts upon this reagent.

Although the nature of the salts contained in sea water may be in some measure known from these preliminary experiments, we thought it necessary to be particularly careful in determining the quantities of saline matter by analysis.

We introduced 1000 grammes (15444 grains) of each of the specimens of sea water into a retort furnished with a bent tube plunging into lime water. There was first disengaged the air of the vessels, and a quantity of air which existed in the sea water. Then there passed off carbonic acid gas; but not till the sea water became boiling hot. Carbonate of lime was formed, which weighed 7.7 grains. This quantity of carbonate of lime shows us that the sea water contained 3.5 grains of carbonic acid gas.

A glass receiver was now applied to the retort, and the distillation was continued till half a litre (30.5 cubic inches) of liquid came over. The product obtained in this way from each specimen of sea water was rendered muddy by nitrate of silver and acetate of lead. It contained neither muriate of lime nor muriate of magnesia; for the smallest quantity of these salts has the property of giving a green colour to the syrup of buckthorn, which was not the case with this product. Hence it would appear that a portion of muriate of soda is volatilized during the distillation. A depot is formed during the cooling, which consists merely of the earthy carbonates, of which we shall speak below.

The evaporation of the water was finished in a basin of silver, and the residue was dried at the temperature of boiling water. The quantities obtained were as follows :—

Water of the English Channel .... 36 grammes
——————— Atlantic 38
——————— Mediterranean 41

A hundred grammes of these different salts exposed for three days to the open air attracted moisture, and were increased in weight to 116 grammes, and this weight was still farther increased by longer exposure.

This saline residuum was repeatedly digested in alcohol, in order to dissolve the deliquescent salts. We continued the digestions till the alcohol ceased to be rendered muddy by potash, which showed the absence of the deliquescing earthy salts. Boiling alcohol must not be employed; because it has the inconvenience of dissolving a quantity of the common salt, a part of which indeed crystallizes as the liquid cools. The salts ought to be reduced to a fine powder, otherwise a portion of the deliquescent salts might be left undissolved.

The different alcoholic liquids were evaporated to dryness. The dry matter was exposed to the air, and in a few days it deliquesced into a liquid. Then two decigrammes (three grains) of common salt were separated from it, and added to the undissolved portion of the salt. The liquids were then again evaporated to dryness, and the residues dried at the temperature of boiling water.*

The dry residues were then weighed. The following were the results obtained:—

Water from Dieppe—Muriate of magnesia, 7 grammes.
Water from Havre—Muriate of magnesia, 7 grammes.
Water from Bayonne—Muriate of magnesia, $6\frac{1}{2}$ grammes.
Water from Marseilles—Muriate of magnesia, 7·3 grammes.

These different salts, when treated with potash, yielded from 1·3 gramme to 1·6 gramme of magnesia.

The small quantity of magnesia obtained is a proof that the salt contained too much water. Hence we think it more exact to determine the weight of the salt from that of the magnesia dried and calcined. This would indicate for the water of the Channel and of the Atlantic 3·5 of muriate of magnesia, and for the water of the Mediterranean 5·25.

When the residue of the evaporation of a kilogramme (15444 grains) of water has been thus exhausted by alcohol and dried, it no longer attracts humidity from the atmosphere.

We ascertained that this salt dissolved by alcohol contained no muriate of lime. Indeed it is obvious that no such salt can exist in sea water if we attend to the presence of sulphate of magnesia. Muriate of lime when dropped into sea water occasions a precipitate when the water has been concentrated to one sixth of its original volume, as we have ascertained by experiment. The salt dissolved by the alcohol is pure muriate of magnesia, which is alone the cause of the deliquescence of sea salt.

The salt thus digested in alcohol, and dried, was thrown into boiling water. The solution was not complete. There remained a

* The salt must not be exposed to too high a temperature, otherwise a portion of the muriate of magnesia will be decomposed.

white powder, which, separated, washed, and dried, weighed 0·3 gramme. The insoluble residue from the water of the Channel weighed 0·4 gramme.

Muriatic acid dissolved a part of this residue with effervescence. The insoluble portion was sulphate of lime, and weighed 0·15 gramme.

Ammonia poured into the filtered liquid occasioned a white precipitate, which was not acted on by potash, but was dissolved by sulphuric acid. We ascertained that this muriatic solution contained lime, magnesia, and a trace of iron.

Thus the insoluble matter is composed of

> Sulphate of lime ....0·15 gramme
> Carbonate of magnesia and of lime ..0·15
> With a trace of iron.

After having determined the nature of the insoluble matter, we examined the liquid which contained the soluble salts, and particularly the muriate of soda in solution.

The quantity of sulphate of magnesia was determined in the following manner. Potash was poured into the solution as long as it formed any precipitate. The liquid was filtered while still hot, and the precipitate separated, washed, dried, and calcined. We obtained 2·2 grammes of magnesia, corresponding to 5·78 grammes of sulphate of magnesia.

If instead of potash we employ nitrate of barytes, we obtain a precipitate of sulphate of barytes, which, after being washed and calcined, contains exactly the quantity of sulphuric acid necessary to saturate the magnesia. Hence it follows that the soluble sulphate is entirely sulphate of magnesia, and that sea water contains no sulphate of soda.

To determine with still greater accuracy whether sea water contains any sulphate of soda, we calcined a portion of the salt in a crucible with purified charcoal, obtained from lamp-black. The residual mass was mixed with water, and filtered. Paper stained with turmeric was not altered by this solution, which shows that it contained no alkali. Nothing was found but a little hydrogureted sulphuret of magnesia. The greatest part of the magnesia remained on the filter with the charcoal.

It is well known to be scarcely possible to separate sulphate of magnesia completely from common salt by crystallization; because the difference between the solubility of the two salts is not sufficiently great. Neither does efflorescence furnish a certain method. We exposed the two salts to the gentle heat of a stove. The sulphate of magnesia effloresces very slowly, and the efflorescence is not so sensible as when sulphate of soda is crystallized confusedly with common salt.

By subtracting all the separated salts from the total mass, we obtain the weight of the common salt. But that method did not

appear to us sufficiently exact. We thought it better to take all the soluble salts obtained from a litre of sea water from which the deliquescent muriates had been separated by alcohol. Nitrate of silver was dropped into the solution previously very much diluted, as long as any precipitate appeared. The muriate of silver obtained was washed and dried ; its weight was 62 grammes, which corresponds with 24·8 grammes of common salt. If to this we add the 0·3 gramme of common salt separated from the alcoholic solution of the muriate of magnesia, we obtain a total of 25·1 grammes of pure muriate of soda.

From the experiments above detailed, we consider ourselves as warranted to conclude,

1. That a small quantity of the common salt contained in sea water is carried off by distillation. This explains why, at a certain distance from the sea, we find muriate of soda on the vegetation.

2. That sea water contains no muriate of lime.

3. That it contains no sulphate of soda.*

4. That muriate of magnesia is the only deliquescent salt in sea water. From it comes the property which impure common salt has of deliquescing in the air.

5. That the water of the English Channel contains a little more carbonic acid than that of the Mediterranean, owing probably to its being of a lower temperature.

The following table exhibits the constituents of these different specimens of sea water, according to the result of our analyses :—

Names of the specimens.	Weights.	Result of evaporation.	Carbonic acid gas.	Common salt.	Muriate of magnesia.	Sulphate of magnesia.	Carbonate of lime and magnesia.	Sulphate of lime.
	Gram.	Gram.	Gram.	Gram.	Gram.	Gram.	Gram.	Gram.
Water of the Channel.	1000	36	0·23	25·10	3·50	5·78	0·20	0·15
Ditto Atlantic........	1000	38	0·23	25·10	3·50	5·78	0·20	0·15
Ditto Mediterranean..	1000	41	0·11	25·10	3·25	6·25	0·15	0·15

* We have examined various specimens of the common salt of commerce, without finding in it any muriate of lime or sulphate of soda. The muriate of magnesia existed in smaller quantity than in the salt obtained by evaporating sea water. If the salts which we examined came from sea water, we must suppose that the muriate of magnesia having deliquesced was partly absorbed by the sand over which the water was evaporated.

Hence in order to imitate sea water, whether taken internally or for bathing, it is not sufficient simply to dissolve common salt of commerce in water. The muriate of magnesia may have some action on the animal economy. It appears to us, therefore, that in order to be exact in the imitation of sea water, it would be necessary to employ the proportions of salts pointed out by us in a subsequent part of this paper.

Utility of Sea Water in a Medical Point of View.

The advantage of a sea voyage in the cure of consumption has been boasted of with reason.

Russel, an English physician, has written a treatise in which he has enumerated all the advantages of sea water taken internally to procure periodical evacuations. He shows that it is of the utmost importance in affections of the glands, whether of the lungs or the mesentery; and likewise against all cutaneous diseases, provided they have not come the length of abscesses. His method of proceeding is to make it act as a laxative, and to attenuate by degrees the humour in the vessels of the glands, in order afterwards to get rid of it altogether. He recommends the use of sea water to facilitate the evacuation of biliary calculi, in cases of obstruction of the liver and jaundice. In such diseases he advises the use of sea water along with soap; but he prohibits the use of this remedy as long as inflammation exists. He thinks he has observed the good effects of sea water in hectic from disorders of the alimentary canal, in scrofula, in the bilious colics with which sailors are afflicted, when we are certain that no inflammation exists; because in that case we must bleed, give laxatives, nitre, and every thing that has a tendency to prevent the formation of an abscess.

Sometimes, when the case is very serious, it may be necessary to apply caustic, blisters, and to keep the bowels open by a quantity of sea water. About a pint of sea water will be sufficient. When this practice is followed by fever, loss of strength, &c. it must be suspended, and asses' milk and absorbents administered; but such cases are of rare occurrence.

When necessary, Ethiop's mineral, cinnabar, antimony, and cooling salts, may be given along with sea water, to facilitate its action.

The ancients were likewise acquainted with the advantages of sea water. Pliny says, *Aquam maris efficaciorem discutiendis tumoribus putant medici quidam et quartanis, dedere eam bibendam in tenesmis.* (Lib. ii. cap. 12.)

Celsus says, *Acris autem est aqua marina, vel alia sale adjecto et utraque decocta commodior est.* (Lib. iii. cap. 24.) And in another place, *Asclepiades aquam quoque salsam et quidam per biduum purgationis causa bibere cogebat.* Hildanus says, that sea water was successfully used in the pestilence.

Hippocrates has recommended injections of sea water. Pliny says, that sea water is good for removing tumors, especially of the parotids, when barley meal is boiled in it.

As to sea water as a bath, we may consider it as a complex bath, as its action must depend upon the properties of the pure water, and of the salts dissolved in it. We may conceive that they penetrate with the water into the tissue of the skin, and produce a greater tonic, operative, and diuretic effect, than pure water would do.

M. de Montegre, editor of the Gazette de Santé, expresses himself thus, relative to bathing in the sea :—It is advantageous to persons of a melancholy or hypochondriacal temperament; in diseases of the liver, the spleen, and the kidnies. It may be considered as a preservative against catarrhs and rheumatisms. The sudden shocks of the waves are a powerful remedy in cases of chlorosis, fluor albus, weaknesses from laborious and premature deliveries, paralysis, &c. The most advantageous season of bathing is from the middle of July to the middle of September.

It has been supposed that in cases of hydrophobia, if the patient be plunged into the sea, a cure will be effected. Perhaps the remedy may be good in recent cases before the symptoms appear ; but it has not been found efficacious in confirmed cases, except when joined with other and much more powerful remedies.

The temperature of sea water when used as a bath is generally from $53\frac{1}{2}°$ to $60°$.

M. Lefrançois, a physician at Dieppe, has published an interesting dissertation on the medical use of sea bathing. Baths have even been established at Boulogne and at Dieppe. Another establishment, similar to those in England, has likewise been formed, under the direction of M. Julieu, physician to the civil and military hospitals.

Those persons who cannot leave their homes may be frequently supplied with an artificial sea water, very similar to the natural.

Mr. Swediaur has proposed one, which may be used either by way of hot or cold bath, in all cases of scrofulous and lymphatic diseases. It is composed as follows :—

Water	50 pounds
Common salt	10 ounces
Muriate of lime	2
Muriate of magnesia..............	$1\frac{1}{4}$
Sulphate of magnesia	$\frac{3}{4}$
Sulphate of soda	$\frac{3}{4}$

This recipe, founded on the old analysis, requires now to be altered. The following is what we, from our own experiments, substitute in its place :—

Water	1 litre
Muriate of soda....................	24 grammes
Sulphate of magnesia	6
Muriate of magnesia..............	4
Sulphate of lime	
Carbonate of magnesia .... ā ā	0·15
Carbonate of lime	

Or, reduced to English weights and measures :—

Water	1 quart
Common salt	370 grains

Sulphate of magnesia .... 92 grains
Muriate of magnesia 61
Sulphate of lime .... 2·3
Carbonate of magnesia ....,. 2·3
Carbonate of lime .... 2·3

All these substances are put into the water, and then a current of carbonic acid gas is passed through it till the earthy salts are dissolved.

This addition of carbonic acid is requisite only when sea water is prescribed internally. When it is to be used as a bath, the addition of the two carbonates may be omitted.

ARTICLE VII.

Analysis of Asbestous Actinolite. By Thomas Thomson, M.D. F.R.S.

THE mineral to I which give this name, because I consider it as possessing the characters of the asbestous actinolite of Werner, occurs in considerable quantity in Huel Unity, about three miles east from Redruth, in Cornwall, and was shown me by the overseer as a substance with the nature of which he was not acquainted. It possesses the following characters :—

The colour is greenish grey, or rather greenish black, and here and there it looks as if powdered with a substance of a lemon yellow colour.

It occurs in large masses, which consist entirely of a congeries of capillary crystals.

The lustre is glistening and silky.

The fracture, on account of the smallness of the crystals, cannot be determined. That of a large mass appears fibrous from the capillary crystals, which, however, cross each other in every direction.

For the same reason the fragments are sharp edged.

It is opake.

It appears soft, as it may be easily crumbled to powder between the fingers; but this is owing to the loose manner in which the crystals are attached to each other; for if we try an individual crystal we shall find it difficult to scratch it with the point of a knife.

Brittle.

Specific gravity 2·916, at the temperature of 50°.

1. Fifty grains of this mineral, being reduced to a fine powder, were exposed to a strong red heat in a platinum crucible. The loss of weight amounted to 0·850 grain, and was ascribed to the evaporation of moisture.

2. Fifty grains of the same mineral in the state of a fine powder were fused with twice their weight of soda in a platinum crucible, and kept in a red heat for an hour. The crucible was then taken from the fire. The colour of the fused mass was brown; but when cold it was partly green and partly brown. It was softened with distilled water till the whole was detached from the crucible.

3. The watery liquid, on standing, deposited a copious reddish yellow sediment, and became itself colourless. Five eighths of this liquid were separated from the rest, and neutralized as exactly as possible by means of nitric acid. Into this neutralized portion some recently prepared nitrate of mercury was poured. A precipitate fell, which, being washed, dried, and exposed to a red heat, weighed 1·2 grains. Hence it is obvious that if the whole watery liquid had been treated with mercury the precipitate obtained would have weighed 1·92 grains.

This precipitate had a yellowish colour; it was tasteless, and insoluble in water; but it dissolved in nitric acid, muriatic acid, and in potash ley, and even partially in ammonia. The alkaline solutions were colourless. When the acid solution was mixed with nitrate of lead, a white precipitate fell. Prussiate of potash indicated the presence of a little iron. Infusion of nutgalls occasioned a white precipitate, which was redissolved by agitation. These were all the trials which the small quantity of precipitate in my possession enabled me to make. They do not enable us to determine with certainty the nature of the substance; but we see that it was not chromic acid. I am disposed to consider it as tungstic acid. At any rate, it agrees more nearly with the properties of that substance than with any other with which we are acquainted.

4. The remainder of the aqueous solution, at the bottom of which was the copious reddish yellow sediment, being treated with muriatic acid, a complete solution was obtained, which had a yellow colour, and was transparent. When mixed with that portion that had been neutralized by nitric acid, and treated with nitrate of mercury, a white precipitate fell, which was calomel. It was owing to the combination of the mercury with the muriatic acid. This precipitate was got rid of by filtration.

The muriatic solution was now concentrated by evaporation. It gradually assumed the form of a jelly. Being evaporated to dryness, and digested in distilled water acidulated with muriatic acid, the whole was thrown upon a filter. The silica remaining on the filter being edulcorated, dried, and heated to redness, weighed 16·7 grains.

5. The liquid which passed through the filter had a green colour. It was supersaturated by carbonate of soda, in order to throw down the whole of its earthy and metalline constituents. The precipitate was allowed to subside to the bottom of the vessel. The colourless liquid was then decanted off, water substituted in its place, and the whole thrown on a filter. The matter collected on the filter had a brownish yellow colour. Before it was quite dry it was mixed with

soda ley, and heated in a porcelain vessel, in order to dissolve any alumina which it might contain. The matter by this treatment assumed a much darker colour, and diminished considerably in bulk. The whole was now allowed to cool, and then poured upon a filter, and the matter which remained upon the filter was sufficiently edulcorated with water.

The liquid which passed through the filter, being mixed with a solution of sal ammoniac, became milky, and deposited the alumina which it had dissolved. This alumina, being washed, dried, and heated to redness, weighed 14·1 grains.

6. The matter remaining on the filter had a reddish brown colour. It was dissolved in very dilute sulphuric acid, the solution was evaporated to dryness, and redissolved in distilled water. The undissolved portion, being heated to redness, weighed 0·6 grain, and was sulphate of lime, equivalent to 0·25 lime.

Suspecting that the liquid might still contain some sulphate of lime, I evaporated it down till it became concentrated, and then mixed it with a quantity of weak alcohol. A number of needle-form crystals separated, which were sulphate of lime; and after being washed and heated to redness, weighed 0·65 grain, equivalent to 0·273 grain lime; so that the whole lime obtained amounted to 0·523 grain.

7. The sulphuric acid liquid thus freed from lime was made as neutral as possible by means of carbonate of ammonia, and then succinate of potash was dropped in as long as any precipitate fell. The red precipitate thus obtained weighed, when dry, 26·2 grains, and was succinate of iron. Being exposed to a red heat in a platinum crucible, its weight was reduced to 8·375 grains, which was red oxide of iron.

8. The liquid, though thus freed from iron by succinate of potash, was not quite limpid, but had a dirty brownish yellow tinge, which made me suspect that it still retained some iron in solution, though succinate of potash had ceased to produce any farther effect. On being mixed with carbonate of soda, and boiled, a white precipitate fell, which, being heated to redness, weighed 4·1 grains. This precipitate was at first white, but became darker on drying, and when heated to redness became reddish brown. It was, therefore, principally manganese.

9. To ascertain if it contained any magnesia, I digested it in nitric acid, filtered the solution, evaporated it to dryness, and digested the dry mass in distilled water. There remained undissolved 0·2 grain of a reddish matter, which proved to be oxide of iron. The aqueous solution, being mixed with carbonate of soda, and heated, let fall a white precipitate, which weighed 0·3 grain, and was magnesia.

10. The liquid from which the manganese was precipitated had a blue colour, and gave a red precipitate with prussiate of potash; therefore it contained copper. I supersaturated it with muriatic

acid, and threw down the copper by a plate of zinc. It weighed 0·5 grain.

11. The constituents thus obtained not amounting to the weight of the mineral analysed, it remained to examine whether it contained an alkali. For this purpose it was fused with five times its weight of nitrate of barytes. It was then dissolved in muriatic acid, and filtered, to separate the silica which remained undissolved. The muriatic solution was mixed with sulphuric acid, and filtered, to get rid of the barytes. The liquid was then supersaturated with carbonate of ammonia, to get rid of all the earthy and metallic substances which it held in solution, and the liquid passed through a filter. This liquid was evaporated to dryness, and the dry salt exposed to a red heat in a platinum crucible. There remained 4·3 grains of a substance, which proved on examination to be sulphate of soda. Now the soda contained in 4·3 grains of sulphate of soda is 1·90 grain.

From the preceding analysis it appears that the constituents of the mineral are as follows:—

Silica	16·700	33·4
Alumina	14·100	28·2
Lime	0·523	1·046
Magnesia	0·300	0·6
Oxide of iron	8·575	17·15
———— manganese	3·600	7·2
Tungstic acid	1·920	3·84
Copper	0·500	1·00
Soda	1·900	3·8
Moisture	0·850	1·7
Loss	1·032	2·064
	50·000	100·000

The great number of constituents in this mineral is remarkable. As the mineral came from a copper-mine, I consider the copper as in all probability proceeding from some unnoticed speck of copper ore ; but it was found in the same proportion nearly in three different trials. I suspect the manganese not to have been free from iron. I had undertaken an analysis on purpose to determine that point more correctly ; but an accident destroyed the vessel and its contents before it was in my power to determine the point. From the great number of constituents, one would suspect the presence of various minerals ; but nothing appears to the eye except the yellow specks and the crystals.

ARTICLE VIII.

Analysis of several Mineral, Vegetable, and Animal Substances.
By Dr. John.*

THE first section of this work contains the analysis of the juice
of the euphorbia cyparissias.   It is composed of

Water 77
Tartaric acid .... Trace
Resin .... 13·8
Gum .... 2·75
Extractive .... 2·75
Albumen .... 1·37
Caoutchouc .... 2·75
Oil .... Trace
                               ─────
                               100·42

The earthy parts of the euphorbia are composed of the carbonate,
sulphate, and phosphate of lime.

The analysis of the asclepias syriaca furnished

Resin .... 26·50
An elastic substance .... 12·50
Vegetable gluten .... 4·00
Tartaric acid and albumen .... 53·00
                               ─────
                                96

The plant when incinerated furnished carbonate of potash, phos-
phate of lime, phosphate of magnesia, silica, iron, and oxide of
manganese.

The author next analysed an elastic scarlet coloured substance
which comes from the east through Turkey, and which is known by
the name of caoutchouc of Thibet.   This brilliant and globular
matter serves for bracelets, ear-drops, rosaries, &c. to the Russian
ladies : and the substance which was usually considered as caout-
chouc is only an indurated and oxidated red oil.   Its colouring
matter is analogous to that of stick lac.†

The fruit of the rus typhinum contains gallic acid at the first
moment of its developement ; but as it advances to maturity bitar-
trate of lime is formed ; and as soon as the circulation of the sap in

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* This short paper is translated from the Ann. de Chim. vol. lxxxviii. p. 99,
into which it has been translated from the Gazette Literaire de Jena for March,
1811.   The paper gives an account of a book published by Dr. John.
    † Bucholz has published an analysis of it in the first volume of Schweigger's
Journal.   He found it a concrete fixed oil of a red colour; but could not succeed
in extracting from it a peculiar colouring matter, as announced by John.

it has ceased, acetic acid is formed. This leads to the suspicion that the acetic acid has been formed at the expense of the tartaric.

Galipot, according to our author, is composed of a volatile oil and a resin. According to other authors, galipot is nothing else than a very impure specimen of the resin called *elemi*.

Analysis of Rocou.—The rocou of commerce is a substance already altered by fermentation. The author procured grains of it from which he obtained the following result:—Rocou contains an aroma, an acid, resin combined with the colouring matter, vegetable mucilage, fibrin, coloured extractive, and a peculiar matter which approaches to mucilage and extractive. This analysis explains the reason why an alkali is added to rocou when it is employed in dyeing. The alkali combines with the resin, and forms a soap which dissolves in water. The alkali acts likewise on the colouring matter, and renders it more lively.

The second section of Dr. John's work contains observations on diabetic urine. Though neither urea nor gelatine are to be found in diabetic urine, it notwithstanding contains azote. Diabetic urine is composed of sugar, animal gum, urate of potash, sulphate of potash, muriate of soda and ammonia, phosphate of magnesia, and phosphate of iron.

Some experiments on the excrements of butterflies leads the author to suspect that they contain uric acid.

The third section contains the analysis of some minerals. The *agalmatholite* of China (the *bildstein* of Werner) of a wax yellow colour is composed of

Silica	55
Alumina	30
Lime	1·75
Oxide of iron	1
———— manganese	Trace
Potash	6·25
Water	5·5
	99·50

The red agalmatholite of China was composed of

Silica	51·50
Alumina	32·50
Lime	3·00
Oxide of iron	1·75
———— manganese	1·20
Potash	6 00
Water	5·13
	101·08

The new mineral from the North called *galronite* is composed of

Silica 54
Alumina 24
Magnesia .... 1·5
Oxide of iron and manganese .... 1·25
Water 2
Potash and soda .... 17·25
                                  ———
                                  100·00

The mineral called lytrode is composed of

Silica .... 44·62
Alumina .... 37·36
Lime........................... 2·75
Oxide of iron .... 1·00
Soda .... 8·00
Water.......................... 6·00
Magnesia and manganese .... Trace
                                  ———
                                  99·73

The mineral called *razoumoffskin* is found at Kosemutz, accompanied with pimelite and chrysoprase. It is composed of

Silica 50
Alumina .... 16·88
Water 20
Oxide of nickel .... 0·75
Magnesia, oxide of iron, and lime 2
Potash .... 10·37
                                  ———
                                  100·00

The zircon found at Friedrschwaerm, in Norway, is composed of

Zirconia........................ 64
Silica 34
Oxide of titanium 1
Oxide of iron.................... 0·25
                                  ———
                                  99·25

Earthy wavellite is composed of

Alumina .... 81·7
Water.......................... 13·5
Lime .... 4·0
Magnesia .... 0·83
Potash......................... 0·5
                                  ———
                                  100·53

Article IX.

Memoir on Palladium and Rhodium. By M. Vauquelin.*

I. Palladium.

Historical Sketch of the Facts hitherto ascertained respecting Palladium.

In the month of April, 1803, a printed note was circulated in London, stating that a new metal resembling silver was sold by Mrs. Forster, Gerrard-street, under the name of *palladium*, or new silver. The following properties of the metal were announced in the printed note:—

1. It is soluble in nitric acid, and gives a solution of a deep red.

2. Sulphate of iron precipitates it from that solution, as it does gold from nitromuriatic acid.

3. If the solution be evaporated, we obtain a red oxide, which is soluble in muriatic acid, and other acids.

4. It is precipitated by mercury, and by all the other metals, excepting gold, platinum, and silver.

5. Its specific gravity, when hammered, is only 11·3 ; but after being rolled out, it is 11·8.

6. Its surface becomes tarnished by an ordinary red heat, and it acquires a blue colour ; but it recovers its brilliancy when violently heated, as is the case with the other noble metals.

7. The strongest heat of a forge is scarcely sufficient to melt it ; but if a little sulphur be added to it while hot, it melts, and flows as readily as zinc.

Mr. Chenevix, who was one of the first that received this printed notice, suspecting, from the odd manner of publishing so interesting a discovery, without the name of the author, some error or imposition, immediately procured a quantity of the metal.

After having recognised in it the characters that had been assigned, Mr. Chenevix, satisfied that it did not resemble any known metal, engaged in a laborious set of analytical and synthetical experiments on the subject; the result of which he communicated to the Royal Society, on the 12th of May, 1803.

These researches led the author to conclude that palladium was a compound of platinum and mercury ; for having obtained palladium by precipitating a mixture of the solutions of platinum and mercury by sulphate of iron, he took it for granted that he had formed it. But there can be no doubt, from what we know of the subject at present, that all the palladium which he obtained had been contained in the platinum solution.

Dr. Wollaston, by examining the solution of platinum, found in

it not only palladium, but likewise another metal, till then unknown, to which he gave the name of *rhodium*, from the red colour of some of its salts. He published an account of its principal properties in the Philosophical Transactions for 1804. Dr. Wollaston employed the following method to separate these two metals from a solution of platinum. He dissolved in nitromuriatic acid two ounces and a half of crude platina; he precipitated the platinum by means of sal ammoniac from a portion of that solution corresponding to 1000 grains of the ore. Into the residual liquor he put a plate of zinc, in order to precipitate from it all the metals which it held in solution. But as he found both copper and lead present, as well as palladium and rhodium, he separated them by means of weak nitric acid, which does not act upon the two last metals.

He dissolved the residue in nitromuriatic acid, and precipitated the platinum still remaining by means of sal ammoniac. To the solution thus precipitated he added 20 grains of common salt, evaporated to dryness, and washed the residual mass with alcohol till that liquid came off colourless. By this method he dissolved the triple salt of palladium, but left that of rhodium.

Though Dr. Wollaston only operated on 1000 grains of the ore of platinum, and of course had only six or seven grains of the new metals at his disposal, yet he determined their principal properties, which does infinite honour to his sagacity; for the thing appears at first view incredible.

For my part, though I employed 60 marcs (about $39\frac{1}{3}$ troy pounds) of crude platina, I found it very difficult to separate exactly the palladium and rhodium from the platinum, and the other metals which exist in that ore, and especially to obtain them in a state of purity.

II.

Before describing the method which I followed to separate palladium and rhodium in a state of purity from the ore of platinum, I conceive it will be worth while to point out certain conditions favourable to the solution of the ore of platinum, and to the precipitation of that metal in the state of a triple salt.

The first relates to the proportion of acids which ought to compose the aqua regia. That which appeared to me the best is one part of nitric acid and two parts of muriatic acid. Aqua regia thus composed not only dissolves more platinum, but its price is nearly 100 per cent. less than that of common aqua regia; and when we operate upon large quantities, this is a point by no means to be overlooked.

The second observation relates to the concentration of the acids. The quantity of nitromuriatic acid necessary to dissolve platinum decreases not merely in proportion to its concentration, but in a much greater ratio; for example, if we suppose a volume of nitromuriatic acid represented by two, to be capable of dissolving a

certain quantity of platinum; if that volume be reduced to one, it will be much more capable of producing the effect than it was before. Thus an aqua regia composed of two parts of muriatic at 22°, and one part of nitric acid at 34°, and which marks about 25° on the areometer, will dissolve about $\frac{1}{8}$th of its weight of platinum, while an aqua regia composed of the same proportion of muriatic acid at 22°, and of nitric acid at 44°, and marking 28·5° on the areometer, will dissolve the fourth of its weight of platinum. Hence one half of the quantity of this last aqua regia would dissolve the same weight of platinum as the first; but one part of this last acid costs less than two parts of the first. We have this advantage besides, that the solution with the strong acid is much more rapid than with the weak, and hardly requires any other heat than what is naturally developed during the process.

It will be easy to understand the reason of this when I observe that nitromuriatic acid of 18° or 20° does not attack platina without the assistance of heat, and even acts upon it very slowly at a boiling heat; but at such a temperature a great proportion of the acid is evaporated before it has time to act upon the metal at all. Hence the reason that the acid collected during the solution of platinum, even when reduced to the consistence of a thick syrup, does not act upon platinum again. Hence also the reason that by means of weak aqua regia we may dissolve the gold mixed with platinum without touching the platinum itself.

The third observation relates to the state in which the solution of platinum ought to be in order that the metal may be completely precipitated by sal ammoniac. It ought to be sufficiently concentrated by evaporation to assume a crystalline state on cooling; for if it contains a great excess of acid, it retains much more triple salt in solution after the addition of the sal ammoniac. If into such a solution we add a portion of alkali to saturate a portion of the superabundant acid, a new precipitate of triple salt takes place. It is well known too that ammonio-muriate of platinum is much more soluble in water acidulated by aqua regia than in pure water. I discovered this by putting into the liquid from which ammonio-muriate of platinum had been separated iron plates to precipitate the metals which still remained in solution. The first effect produced was the precipitation of a considerable quantity of triple salt of platinum almost pure; but what embarrassed me at first respecting the true cause of this precipitation was that sulphate of iron produced the same effect as metallic iron.

This made me suspect that in the solution of platinum that metal might be in two states of oxidation; that the protoxide alone was precipitated by sal ammoniac, while the other that remained in solution was precipitated in its turn by yielding a portion of its oxygen to the iron. To verify this conjecture, I passed a quantity of oxymuriatic acid through a solution of triple salt of platinum to peroxidize the whole metal, if that should be possible.

The gas dissolved in the liquid very readily, heat was evolved,

and the liquid assumed a red colour, while at the same time azotic gas was disengaged; but when I put iron into this solution, it precipitated no salt, but platinum in the metallic state. Sulphate of iron produced no effect; while sal ammoniac and ammonia itself precipitated a great quantity of yellow salt. Thus oxymuriatic acid did not alter the state of oxidation of the metal; but decomposed the ammonia, and thus reduced the triple salt to the state of a simple muriate. Hence it follows that sulphate of iron, as well as iron itself, has the property of saturating a portion of the acid which holds the triple salt in solution. The sulphate of iron being decomposed by the muriatic acid, it is possible that the sulphuric acid set at liberty has not the same dissolving power on the triple salt that the muriatic acid had.

The fourth observation which I shall make is to dilute the solution of the metal with a quantity of water. Without this precaution it would be very difficult to wash the precipitate, and it would remain mixed with iron and with the other metals that happen to be present. It is better that the whole platinum should not be precipitated than that the precipitate should be impure; because the platinum remaining in solution is separated in the subsequent processes.

Ten parts of water and one of the solution in a state of great concentration appear to me to be the best proportion. The solution of sal ammoniac employed to precipitate ought to be saturated.

Ammonio-muriate of platinum is not pure unless it has a lemon yellow colour, does not become brown in drying, and is easily reduced to powder.

III.

Method of separating the Palladium and Rhodium, and the other Metallic Salts which are united in the same Solution.

After having precipitated the platinum by means of sal ammoniac, I put into the mother liquor plates of iron, in order to precipitate the different metals which accompany platinum in its ore.

I treated the black precipitate which I obtained successively with cold nitric and muriatic acids. When these two acids ceased to act I washed the residuum, and dried it. During this operation very acrid white vapours rose; which I ascertained, by heating a portion of the residuum in a crucible, to be a mixture of calomel and muriate of copper. The sublimate contained likewise globules of mercury, and a black matter, which I suppose, from the smell which it exhaled, to be osmium.

The nitric acid which had been digested on the precipitate contained much iron, some copper, and a small quantity of palladium, though the digestion had taken place at the common temperature of the air.

The muriatic acid employed after the nitric contained likewise a great deal of iron, some copper, and palladium, and even platinum

and rhodium. This shows that a portion of these last three metals is precipitated by the iron in the state of oxide; otherwise muriatic acid would not have dissolved them. This seems to prove likewise that these metals in precipitating combine with iron and copper, and prevent them from being attacked by nitric acid, even when employed in considerable quantity.

I next treated the precipitate with nitromuriatic acid composed of the acids of commerce. A part only of the matter was dissolved. There remained a considerable quantity upon which the acid had no action, though I employed at least 12 parts of acid.

Supposing that this acid was too weak to act with efficacy upon this matter, I composed a very strong nitromuriatic acid, and poured six parts of it upon one of the residue. The action was now very violent, and the solution went on plentifully without the application of heat, as was evident from the strong effervescence, the escape of nitrous fumes in abundance, the elevation of the temperature, and the change in the colour of the liquid. Yet the whole matter was not dissolved. There remained at least $\frac{1}{6}$th, though heat had been applied for a long time.

I therefore poured four more parts of the same nitromuriatic acid on it. There was still solution; but it was less than what took place at first. I will leave this residue at present, but I shall return to it hereafter.

Though the matter thus treated three times successively with nitromuriatic acid had been previously subjected to the action of nitric and muriatic acids, each of the three nitromuriatic acid solutions contained still copper and iron. This shows that these metals precipitated in combination with platinum and the other metals mixed with it. It was by this union that they were protected from the action of the simple acids, and even of the first portions of the nitromuriatic acid.

I had already observed these phenomena in a preceding process; but as I had calcined the precipitate so as to agglutinate the parts, I considered that as the cause of the union which prevented the solution of the iron and copper. But the preceding result shows us that this union takes place at the instant of precipitation. The great quantity of oxide of iron which precipitates along with the platinum, palladium, and rhodium, is very remarkable.

The three solutions of the precipitate were mixed together, and evaporated to the consistence of a syrup, to get rid of the excess of acid. Being then sufficiently diluted with water, it was precipitated by sal ammoniac. A pure yellow ammonio-muriate of platinum fell down.

The liquid being evaporated anew almost to dryness, and the residue being digested in water, left a granular salt of a red colour, similar to the blossom of the pomegranate; but little soluble in water, and composed chiefly of platinum. I shall state hereafter the cause of its red colour.

After this long but necessary preface, I shall state the method

which I employed to separate the palladium from the rhodium, and the latter from the iron and copper with which it is always mixed.

I dilute the liquid above described and deprived of most of its platinum with water, and I add muriatic acid, if there be not already a sufficient excess of it. I then gradually add ammonia, but in such quantity as not entirely to saturate the excess of acid. I then agitate the mixture. There immediately appear a g at number of needle-form crystals, very brilliant, and of a fine red colour.

I allow them to fall to the bottom, and add a few drops of ammonia to a portion of the liquid. If it give crystals as at first, I add a new portion of that alkali to the liquid, and continue to do so till it ceases to furnish crystals.

I then decant off the clear liquid, and wash the precipitate, first with cold water, and then with hot water, which may be done without risk, as it is scarcely soluble. If by accident the proper point of saturation has been passed, and some salt of iron or rhodium is mixed with this precipitate, it is easily freed from it by digesting the precipitate for a few minutes in water acidulated with muriatic acid. This salt is an ammonio-muriate of palladium containing a minimum of acid, the properties of which I shall describe below. To obtain the metal from it, nothing more is necessary than to heat it red-hot.

Now to obtain the rhodium, I concentrate the liquids from which the palladium has been separated, till they crystallize totally on cooling. I put the crystals aside to drain. They are frequently of two kinds and of two colours. Some have the form of hexahedral plates, and a fine ruby red colour; while others, fewer in number, are square prisms of a yellowish green colour. These last are ammonio-muriate of palladium. The water which flows from these crystals has a yellowish green colour, owing to copper and iron which it holds in solution.

The crystals being drained, 1 pound them in a porcelain mortar, and wash them with alcohol of 36°, which I leave over them for 24 hours in a close flask, agitating the mixture frequently. When the yellowish green colour which the alcohol acquires does not increase, it is decanted off, and fresh alcohol added; and this process is continued till the alcohol ceases to acquire any colour, and till it ceases, when examined by re-agents, to exhibit any traces of iron or copper.

If any portion of palladium remained mixed with the rhodium, it would be dissolved in the alcohol added towards the end of the process, and this alcohol would yield by spontaneous evaporation very long square prisms of a greenish yellow colour. We sometimes observe among these crystals small ruby red crystals; they consist of ammonio-muriate of rhodium dissolved by the alcohol.

The salt of rhodium is then dried in the open air. It is of a fine red colour. As it may still contain some portion of the triple salt of platinum, it is to be dissolved in a little water, which may

be acidulated with muriatic acid. The salt of platinum, if there be any, will remain at the bottom of the liquid, and may be separated by the filter.

To obtain the rhodium in the metallic state, the solution containing its triple salt is to be evaporated to dryness, and the dry residue heated in a crucible. It leaves a white metal in a brittle spongy mass, the properties of which will be described hereafter.

This process, more simple and more exact than that of Wollaston, depends, 1. On the insolubility of ammonio-muriate of palladium, even in water slightly acidulous. 2. On the solubility of the muriates of copper and iron in alcohol, and the insolubility of ammonio-muriate of rhodium in the same liquid.

(*To be continued.*)

ARTICLE X.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32' 40.3" North.   Longitude West in Time 6"$\frac{8\,2}{1\,0\,0}$.

1814.

Month.	Morning Observ.				Noon Observ.				Evening Observ.			
	Hour.	Variation.			Hour.	Variation.			Hour.	Variation.		
July 18	8ʰ 35'	24°	10'	30"	1ʰ 45'	24°	26'	00"	6ʰ 45'	24°	16'	50"
Ditto 19	8 50	24	12	26	1 55	24	22	45	6 55	24	14	06
Ditto 20	8 45	24	16	15	1 25	24	23	50	7 00	24	16	12
Ditto 21	8 45	24	13	30	1 50	24	24	02	6 47	24	17	25
Ditto 22	8 40	24	11	15	2 10	24	22	35	7 00	24	17	10
Ditto 23	8 45	24	12	50	1 55	24	22	20	7 05	24	17	32
Ditto 24	8 35	24	13	21	1 35	24	23	34	7 00	24	18	02
Ditto 26	8 30	24	13	30	1 50	24	23	36	6 45	24	16	08
Ditto 27	8 35	24	15	00	1 55	24	23	46	6 55	24	16	24
Ditto 28	8 20	24	13	07	1 40	24	21	46	7 00	24	16	47
Ditto 29	8 40	24	15	55	1 40	24	23	55	6 55	24	15	40
Ditto 30	8 45	24	14	14	1 35	24	24	24	7 08	24	16	28
Ditto 31	8 35	24	12	06	1 45	24	23	22	— —	24	—	—

1814.

Mean of Observations in July.	Morning	at	8ʰ 41'	..... Variation	24°	13'	29"	West.
	Noon	at	1 42	..... Ditto	24	23	44	
	Evening	at	6 58	..... Ditto	24	17	00	
Ditto in June.	Morning	at	8 44	..... Ditto	24	13	10	West.
	Noon	at	1 30	..... Ditto	24	22	48	
	Evening	at	6 52	..... Ditto	24	16	29	
Ditto in May.	Morning	at	8 45	..... Ditto	24	13	12	West.
	Noon	at	1 44	..... Ditto	24	22	13	
	Evening	at	6 38	..... Ditto	24	16	14	
Ditto in April.	Morning	at	8 45	..... Ditto	24	12	53	West.
	Noon	at	1 48	..... Ditto	24	23	53	
	Evening	at	6 29	..... Ditto	24	15	30	

Ditto in March.	Morning	at	8ʰ	52′	.....Ditto	24°	14″	29′	West.
	Noon	at	1	52	.....Ditto	24	23	08	
	Evening	at	6	11	.....Ditto	24	15	33	
Ditto in Feb.	Morning	at	8	47	.....Ditto	24	14	50	West.
	Noon	at	1	52	.....Ditto	24	20	58	
	Evening	at	—	—	.....Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8	52	.....Ditto	24	15	05	West.
	Noon	at	1	53	.....Ditto	24	19	03	
	Evening	at	—	—	.....Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8	53	.....Ditto	24	17	39	West.
	Noon	at	1	51	.....Ditto	24	20	30	
	Evening	at	—	—	.....Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	40	.....Ditto	24	17	17	West.
	Noon	at	1	54	.....Ditto	24	20	24	
	Evening	at	—	—	.....Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	.....Ditto	24	15	41	West.
	Noon	at	1	59	.....Ditto	24	22	53	
	Evening	at	—	—	.....Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	.....Ditto	24	15	46	West.
	Noon	at	2	02	.....Ditto	24	22	32	
	Evening	at	6	03	.....Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	.....Ditto	24	15	55	West.
	Noon	at	2	02	.....Ditto	24	23	32	
	Evening	at	7	05	.....Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	.....Ditto	24	14	32	West.
	Noon	at	1	50	.....Ditto	24	23	04	
	Evening	at	7	08	.....Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	.....Ditto	24	12	55	West.
	Noon	at	1	33	.....Ditto	24	22	17	
	Evening	at	7	04	.....Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	.....Ditto	24	12	02	West.
	Noon	at	1	37	.....Ditto	24	20	54	
	Evening	at	6	40	.....Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	.....Ditto	24	09	18	West.
	Noon	at	0	59	.....Ditto	24	21	12	
	Evening	at	5	46	.....Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
Aug. 1	8ʰ 37′	24° 13′	23″	1ʰ 35′	24° 24′	25″	7ʰ 05′	24° 18′	26″
Ditto 2	8 55	24 20	58	1 50	24 22	47	7 07	24 18	18
Ditto 3	8 35	24 12	56	1 40	24 24	31	—	—	—
Ditto 4	8 30	24 12	44	1 50	24 26	16	7 05	24 14	27
Ditto 5	8 30	24 13	48	1 35	24 22	30	7 15	24 16	16
Ditto 6	8 23	24 15	57	1 30	24 23	24	6 55	24 15	31
Ditto 7	8 45	24 13	30	1 35	24 21	32	6 55	24 15	25
Ditto 8	8 35	24 14	47	1 30	24 23	42	7 05	24 18	41
Ditto 9	8 40	24 13	22	1 37	24 23	12	7 05	24 17	07
Ditto 10	8 28	24 12	59	2 15	24 21	50	7 00	24 16	14
Ditto 11	8 15	24 14	08	1 27	24 23	14	6 55	24 17	00
Ditto 12	8 25	24 14	26	1 35	24 23	59	7 03	24 16	50
Ditto 13	8 15	24 12	29	1 35	24 22	29	6 50	24 15	01
Ditto 14	8 30	24 14	00	1 40	24 23	15	7 10	24 16	27
Ditto 15	8 30	24 12	42	1 45	24 22	31	7 00	24 14	58
Ditto 16	8 40	24 14	54	1 30	24 25	15	7 00	24 17	17
Ditto 17	8 30	24 13	08	1 45	24 26	17	7 05	24 18	31

Comparison of the Variation in the Years 1813 *and* 1814.

		1813.			1814.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	25″
	Noon	24	21	12	24	23	53	+	2	41
	Evening..........	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	24	13	+	1	19
	Evening..........	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening..........	24	16	04	24	16	18	+	0	44
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening..........	24	16	43	24	17	00	+	0	17

Rain fallen $\left\{\begin{array}{l}\text{Between noon of the 1st July}\\ \text{Between noon of the 1st Aug.}\end{array}\right\}$ 0·783 inches.

The following errors have been committed in deducing the mean variation, and are corrected in the present number :—

1813 Dec. Morning Obs.........................*for* 24° 17′ 21″ *read* 24° 17′ 30″
——— Ditto Noon ....*for* 1ʰ 53′ *read* 1ʰ 51′; *for* 24 19 49 *read* 24 20 30
——— Nov. Morning .....*for* 8 42 *read* 8 40; *for* 24 17 42 *read* 24 17 17
——— Aug. Morning ....*for* 24 15 58 *read* 24 15 55
——— July Evening ....*for* 24 13 56 *read* 24 16 43
——— June Morning ....*for* 24 12 35 *read* 24 12 55
——— May Evening .....*for* 6ʰ 14′ *read* 6ʰ 40′

Aug. 2.—The morning observation was unusually great; and the two needles differed from each other 3′ 33″; a remarkable difference.

ARTICLE XI.

ANALYSES OF BOOKS.

Werner's Nomenclature of Colours, with additions, arranged so as to render it highly useful to the Arts and Sciences, particularly Zoology, Botany, Chemistry, Mineralogy, and Morbid Anatomy. Annexed to which are Examples selected from well known Objects in the Animal, Vegetable, and Mineral Kingdoms. By Patrick Syme, Flower Painter, Edinburgh; Painter to the Wernerian and Horticultural Societies of Edinburgh. Edinburgh, 1814. 8vo.

The importance of colour in mineralogy is understood by those only who have paid some attention to the external characters of minerals. Nothing is more common than to hear persons who

possess considerable knowledge of stones declare that colour in mineralogy may be overlooked altogether, and that it contributes rather to throw confusion into the science than to facilitate the investigation of species. Such persons have evidently not paid the requisite attention to the colours of minerals. Their methods of discriminating minerals depend upon the crystalline forms, or some analogous character, and would fail them in many instances, when a person thoroughly acquainted with the external characters would be able to pronounce with confidence and certainty. Suppose a mineral, A, to assume occasionally five different colours, and no more, and that a stone is presented to our view, and the question put whether it belongs to the species A or not; if the colour be neither of the five to which the mineral A is confined, we are enabled at once to answer in the negative, without any farther examination; if the colour be one of the five we must proceed a step farther, and examine whether the fracture, the hardness, the specific gravity, &c. of the mineral under examination, be the same with that of A; if they be, the species of the stone is found without any uncertainty; if they be not, we may pronounce in the negative with equal confidence.

The present work has been long wanted, and mineralogists especially are under great obligations to Mr. Syme for the care and accuracy with which it has been executed. The colours of Werner amount to 79; but Mr. Syme has increased the number in his catalogue to 108, of which there are 13 which have not yet been observed in the mineral kingdom. He has divided them into ten sets; namely, whites, greys, blacks, blues, purples, greens, yellows, oranges, reds, and browns. Werner confounded the purples with the blues, and the orange colours with the yellows; but Mr. Syme, in my opinion at least, has acted properly in placing these two colours into separate sets, as they are as much entitled surely to be considered as distinct colours as the greens, and much more than the greys or browns.

Mr. Syme has also made the following changes on some of Werner's names, to accommodate them better to the terms already in common use in this country:—

> Milk white, changed into Skimmed milk white.
> Blackish lead grey . . . . Blackish grey.
> Steel grey . . . . French grey.
> Smalt blue . . . . Greyish blue.
> Sky blue . . . . Greenish blue.
> Violet blue . . . . Violet purple.
> Plum blue . . . . Plum purple.
> Lavender blue . . . . Lavender purple.
> Orange yellow . . . . Dutch orange.
> Crimson red . . . . Lake red.
> Columbine red . . . . Crimson red.
> Cherry red . . . . Brownish purple red.

The principal part of this little work, and the part to which the whole of its value is to be ascribed, consists in a table of the names of the various colours, with a slip of paper after each name painted of the colour indicated by the name. Then comes a list of some animal, vegetable, and mineral bodies, having the same colour. These references may be of some service to beginners; but they are liable to considerable ambiguity, and may on that account mislead. When Dr. Richardson published his panegyric on fiorin grass, one of his directions was to go to the north side of any church yard wall, and pull up whatever should be found growing there, as it would be no other than fiorin. I have heard of an honest Yorkshire farmer who obeyed these directions to a tittle. He went to the north side of his church-yard, and rooted up a fine crop of hemlock (conium maculatum), which was growing against the wall, and transplanted it with much care to his own farm. His surprise was not small, and his faith in the accuracy of Dr. Richardson's directions not a little shaken when he found afterwards that none of his cattle could be prevailed upon to touch this so much vaunted grass. Young mineralogists may fall into similar mistakes if they take the mineral attached to any colour in an absolute sense. Thus limestone, fluor spar, oliven ore, which are placed after particular colours, occur in so many other colours that a young mineralogist cannot be sure of possessing the colour in question merely by being in possession of a specimen of fluor spar, oliven ore, or limestone, unless he compare the colour of his specimen with that of the slip of painted paper in Mr. Syme's book. The vegetable colours are more uniform and constant, though not free from ambiguity. Thus rose-red is said to be the colour of the common garden rose. Every body knows that there are two varieties of the common rose, differing from each other very considerably in colour. Indeed the term *rose-red* is applied by writers in general with a degree of latitude that renders it very ambiguous. It is applied to the colour of copper, to that of soda-muriate of rhodium (nearly the colour of the unexpanded petals of honey-suckle), and to the colour of the different species of roses. These observations are not meant as attacks upon the plan followed by Werner and Mr. Syme, which may be attended with advantages, and in Mr. Syme's case can occasion little ambiguity; but to prevent the young mineralogist from being misled by a reference to a particular mineral which occurs of other colours besides the one specified.

I do not choose to venture to give an opinion respecting the execution of the shades by a gentleman so much conversant in colours as I know Mr. Syme to be, and so very careful in the execution of all he undertakes, otherwise I should say that the whole of his greys are mixed with rather too much blue, and are deeper than they usually occur in the mineral kingdom.

Article XII.

Proceedings of Philosophical Societies.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(*Continued from* p. 154.)

Physics and Chemistry. By M. le Chevalier Cuvier.

It will be seen in our analysis of 1811 how by accelerating evaporation by a vacuum, and by the presence of a body which very readily absorbs water, Mr. Leslie, of Edinburgh, succeeded in freezing water at all seasons of the year. This philosopher afterwards contrived an apparatus, which was shown to the Class by M. Pictet, a corresponding member, by means of which we may instantly, and at pleasure, either freeze water or restore it again to its liquidity. For this purpose water is placed under the receiver of an air-pump, in a vessel furnished with a lid, which may be lifted up, and let down again, by means of a wire passing through the top of the receiver. When the lid is taken off, the water, by the action of the sulphuric acid and the vacuum, congeals; and when the lid is put on again, the surrounding heat soon restores its liquidity again.

Our associate M. Gay-Lussac, who repeated the experiment of Mr. Leslie before the Class, stated a well known fact of the same kind; namely, the cold produced in certain machines when condensed air is allowed to escape. He has shown that in every season it is sufficient that air be condensed into half its bulk in order to produce ice; and he conceives that ice might easily be procured in this way in hot climates, by condensing air by means of a fall of water.

We may, by employing bodies more evaporable than water, arrive at degrees of cold really astonishing; and not only freeze mercury, but the purest alcohol. This has been done by Mr. Hutton, of Edinburgh; who observed on the occasion, that in the best rectified alcohol, congelation still separates different substances. M. Configliacchi, Professor at Pavia, congealed mercury by the evaporation of water alone. We are indebted for the first communication of these experiments to M. Pictet.

It was supposed that the pressure of air, the influence of which is so powerful in retarding evaporation, would retard also the solution of salts, or, what comes to the same thing, would accelerate their crystallization when they were dissolved: and in fact a satu-

rated solution of Glauber's salt, which preserves its liquidity when cooled in a vacuum, crystallizes as soon as air is admitted.   But M. Gay-Lussac has ascertained that this is far from being the case with other salts ; and that even with respect to sulphate of soda, the phenomenon does not depend upon the presence or absence of the pressure of air.   When the contact of air, for example, is intercepted by a covering of oil, the liquid remains fluid just as well as when it is in a vacuum ; while, on the other hand, the pressure of a column of mercury does not in the least accelerate the crystallization.   A solution which is made to pass through mercury freed from air by boiling, does not crystallize ; but when passed through mercury in its ordinary state, it crystallizes directly.   Agitation, the introduction of a small crystal, and many other causes, determine the crystallization, whatever be the pressure.   Therefore M. Gay-Lussac concludes that it is not by its pressure that air diminishes the dissolving power of water.   He has ascertained, likewise, that it is not by absorbing air that water loses this power; but he thinks it a phenomenon more or less analogous to that of pure water, which, as is known, remains liquid when cooled some degrees below its freezing point, whenever we can prevent it from being agitated ; and congeals the moment it receives the slightest shock.

The most evident source of heat upon the globe is the rays of the sun.   But it has been long remarked that these rays when separated by the prism do not produce equal degrees of heat: and Dr. Herschel, the celebrated astronomer, ascertained some years ago that the heating power goes on increasing from the violet to the red.   He even assures us that beyond the spectrum there are rays, which, though not luminous, have the power of heating more strongly than the red rays.   Messrs. Ritter, Bæckman, and Wollaston, announced soon after that the power of the rays of light to produce certain chemical changes goes in the inverse order, and is strongest in the violet ray and beyond that ray.

M. Berard, a young chemist of Montpellier, who has repeated with much delicacy and precision these two kind of experiments, has ascertained their accuracy in several respects.   He has even found that the chemical power of light goes on diminishing to the middle of the spectrum, and is not sensible beyond that point.   According to him, it is at the extremity of the red ray that the greatest heating power resides, and beyond the spectrum it diminishes.   M. Berard has ascertained, likewise, that these properties belong to light reflected by a mirror, and to light which has passed through Iceland crystal, as well as to direct light.

Equally decisive results have not been obtained respecting the property of magnetizing steel ascribed to the violet ray by M. Morichini, a well-informed Roman chemist.   Although needles exposed to this ray appeared magnetized in certain experiments, they underwent no such change in many other trials; and at present no reason can be assigned for this difference : for in both cases every

other known cause of magnetisement had been carefully removed. The summer of 1813, indeed, was not favourable to these kinds of experiments, it was so bad.

Of all the phenomena presented by heat, the dilatation which it produces in bodies is the one the laws of which may be most naturally expressed by mathematical formulas; and the knowledge of these laws, which constitutes an essential part of natural philosophy, is likewise very important in many chemical experiments. M. Biot has taken a great deal of pains on this subject, and taking as a term of comparison the dilatation of mercury, he finds that the true dilatation of other liquids may be always expressed by that dilatation, by its square and its cube, multiplying each of these three terms by a particular coefficient, which must be determined for each liquid ; but which, when once determined, remains the same at all degrees of heat. As the substance of the thermometer, which contains the liquor observed, undergoes a dilatation itself, the apparent dilatation is different from the true one. But M. Biot shows that it follows a similar law. He then calculates from the experiments of M. Deluc the proper coefficients for eight of the liquids, the dilatations of which it is most necessary to be acquainted with, and shows that these coefficients being once obtained, his formula gives the dilatation for each degree of temperature as accurately as experiment. Finally, he has made the application of them to the combined dilatations of the vessel and the liquid, and shows that we may separate the effects which belong to the liquid and to the vessel, and determine the influence of each with sufficient exactness to find by calculation alone all the results observed : so that henceforth calculation may in a great variety of cases dispense with immediate observations, and we may introduce our calculated results without fear into the elements of phenomena. This is a greater advantage, because this kind of experiment is excessively delicate ; and, unless the utmost possible attention be bestowed, many causes, easily conceived, but almost impossible to avoid, continually interfere with the accuracy of the result.

What M. Charles has observed, in a beautiful set of experiments, made with an instrument of his contrivance to render sensible and to measure the maximum of the dilatation of water, agrees exactly with the formulas of M. Biot, adds to our confidence in them, and shows us that they may be employed with safety.

There has existed for a long time a discussion among chemists respecting the time when alcohol is formed in wine. It was formerly the general opinion that alcohol was a necessary product of fermentation : but M. Fabbroni, a corresponding member of the Institute, has supported the contrary opinion. According to him it is only accidentally, and when there is too much heat, that fermentation produces alcohol. In ordinary wine alcohol is produced only by the heat of distillation. His principal proof was that alcohol could not

be obtained from these wines by means of potash, though by that means we may separate the smallest quantity of alcohol added on purpose to wine.

M. Gay-Lussac has endeavoured to restore the old opinion, by showing that potash shows likewise the quantity of alcohol natural to wine when the liquid has in the first place by means of litharge been freed from the substances which prevented its separation: and that alcohol may be obtained by distilling wine at the temperature of 60°, which is considerably below the heat produced by fermentation.

But it may be alleged that M. Gay-Lussac operated upon wines in which alcohol had been already formed by fermentation, or upon wines into which the merchants had introduced alcohol. To obviate this objection he made himself wine from grapes, and found alcohol in it, as well as in every other wine.*

M. Gay-Lussac has also shown that the absolute alcohol of Richter may be obtained by employing quick-lime or barytes instead of muriate of lime.

Soap, as every body knows, is a combination of an alkali and a fatty matter; but the alteration which the oil experiences by this combination had not been sufficiently examined.

M. Chevreul, Assistant Naturalist to the Museum of Natural History, has employed himself in this inquiry, and has made some new and curious observations. The soap of potash and hog's-lard dissolved in water leaves a pearl coloured substance, which, when separated from the saline matter which it still contains, constitutes a substance possessed of very peculiar properties, which M. Chevreul calls *margarine*, from its pearl colour. It is insoluble in water, but very soluble in hot water. It melts at 133°, and on cooling crystallizes in beautiful white needles. It combines with potash, and then resumes the characters of the pearl-coloured deposite. It has a stronger affinity for that base than carbonic acid has, which it expels from the carbonate of potash by the assistance of a boiling heat. It likewise separates potash from turnsole, and restores it to its red colour.

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* It is singular that no notice is taken here of the experiments of Mr. Brande, published some years ago in the Philosophical Transactions. It would be disgraceful to suppose that none of the Members of the Institute had seen that volume.—T.

(*To be continued.*)

Article XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

St. Thomas's and Guy's Hospitals.—The Winter Course of Lectures at these adjoining Hospitals will commence in the first week of October, viz. :—

At St Thomas's—Anatomy and the Operations of Surgery ; by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery ; by Mr. Astley Cooper.

At Guy's—Practice of Medicine ; by Dr. Babington and Dr. Curry.—Chemistry ; by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy ; by Mr. Allen.—Theory of Medicine, and Materia Medica ; by Dr. Curry and Dr. Cholmeley. —Midwifery, and Diseases of Women and Children ; by Dr. Haighton.—Physiology, or Laws of the Animal Economy ; by Dr. Haighton.—Structure and Diseases of the Teeth ; by Mr. Fox.

N.B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance ; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction.

Dr. Pearson, after delivering Lectures on Chemistry, Therapeutics, with Materia Medica and Medical Botany, but especially on the Theory and Practice of Physic, of each of which three courses were given every year for 26 years, proposes in future to confine himself to the department of the Practice of Physic, with the Laws of the Animal Economy. This course will commence, as usual, the first week of October next, in George-street, Hanover-square. The Medical School will, however, be continued more extensively, in consequence of the united labours of Dr. Roget, Dr. Harrison, and Dr. John Davy, in the department of Therapeutics with Materia Medica, Clinical Practice with Demonstrative Pathology, Medical Jurisprudence, and a full course of Chemistry, to be delivered at the Theatre in Windmill-street.

Dr. Clarke and Mr. Clarke will commence their winter Course of Lectures on Midwifery and the Diseases of Women and Children, on Tuesday, Oct. 4. The lectures are read at the house of Mr. Clarke, No. 10, Saville-row, every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Wednesday, Oct. 5, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street, Blackfriars.

II. *New Application of the Steam Engine.*

Some time ago a steam-engine was mounted upon wheels at Leeds, and made to move along a rail road by means of a rack wheel, dragging after it a number of waggons loaded with coals. The experiment succeeded so well at Leeds, that a similar engine has been erected at Newcastle, about a mile north from that town. It moves at the rate of three miles an hour, dragging after it 14 waggons, loaded each with about two tuns of coals ; so that in this case the expense of 14 horses is saved by the substitution of this steam-engine. I have no doubt that this new employment of the steam-engine will soon become general in coal countries. I was told of another steam-engine at Newcastle, employed for a similar purpose, and moving along, without any rack wheel, simply by its friction against the rail road ; but I had no opportunity of seeing this machine, or of judging of its execution. One would be disposed, however, to give the preference to the engine moved by means of a rack wheel.

III. *Composition of the Vegetable Acids.*

Berzelius has just published the result of his new experiments to determine the composition of the vegetable acids. (Schweigger's Journal für Chemie und Physik, x. 2, p. 246.) I shall lay the numbers which he obtained before the readers :—

Acid	Element	%	atoms
Oxalic Acid	Oxygen	66·211 .. or ..	6 atoms
	Carbon	33·021	4
	Hydrogen	0·728	1
Tartaric Acid	Oxygen	59·882	5
	Carbon	36·167	4
	Hydrogen	3·751	5
Saclactic Acid	Oxygen	60·818	8
	Carbon	34·164	6
	Hydrogen	5·018	10
Acetic Acid	Oxygen	46·734	3
	Carbon	46·871	4
	Hydrogen	6·195	6
Succinic Acid	Oxygen	48·08	3
	Carbon	47·40	4
	Hydrogen	4·52	4
Citric Acid	Oxygen	55·072	3
	Carbon	41·290	3
	Hydrogen	3·634	3
Benzoic Acid	Oxygen	20·00	3
	Carbon	74·74	15
	Hydrogen	5·26	12

These determinations seem liable to some objections ; but as the

details of the experiments are not given, we have no means of judging how far they may be confided in.

IV. *New Acid.*

Gay-Lussac has finished a very laborious and complete investigation of the properties of *iodine.* During his experiments he discovered that chlorine has the property of combining in two proportions with oxygen, and of forming two acids, which he calls the *chloric* and *chlorous* acids. The euchlorine of Davy is Gay-Lussac's chlorous acid ; but it would seem that the chloric acid is the more curious and important compound. I have not heard how it is obtained ; but as soon as the facts are transmitted to us from Paris, I shall lay them before my readers.

V. *New Experiments on the Velocity of Sound suggested.*

I insert the following valuable letter just as I received it :—

(To Dr. Thomson.)

" MY DEAR SIR,

" It has frequently excited my surprise, as well as regret, and in which I am nowise singular, that use has not been made of the admirable trigonometrical survey begun by the late General Roy, and continued, with so much ability and attention, by Col. Mudge and Professor Dalby, to make experiments on the velocity of sound: and however experiments of this kind may have been neglected, it is to be hoped that the present Master General of the Ordnance, a near relation of the late scientific Capt. Phipps (afterwards Lord Mulgrave), so much celebrated for his voyage towards the North Pole, will, for the purpose of perfecting a branch of science no less curious than useful, order a series of experiments of this nature to be undertaken, not only in the inland parts of the kingdom, but also on different parts of the coast. These experiments it would be advisable to make under a variety of circumstances ; for instance, in cold and dry weather ; in windy and calm weather ; in the day time, as well as in the night ; on those days when the air is found to be very elastic, as well as when the atmosphere is found not so well adapted for transmitting sound ; also with heavy and light pieces of ordnance. The places on the coast selected for this purpose might be Dover Castle, Fair Leigh, Beachey Head, Dunnose ; and in the inland parts, which are near the metropolis, perhaps none is better situated than Lady James's Tower at Shooter's Hill ; the facility with which observers can accurately determine the distance of their stations, by taking the angles of the different churches and other objects, renders this spot peculiarly eligible ; and if notice was given of the day on which the experiments were to be made, many philosophical gentlemen would lend their assistance ; and as the pocket chronometers generally make five beats in two seconds of time, the velocity of sound would be determined to

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the fraction of a second. In consequence of peace, the intercourse being open with France, many of our own countrymen and scientific foreigners would assist in ascertaining the time sound is travelling across that part of the Channel where the opposite coasts are visible from each other. An accurate account of the barometer, thermometer, hygrometer, electrometer, as well as of the weather, should be kept on those days the experiments are made, and such remarks inserted in the journal as may be found expedient.

" I remain, my dear Sir, yours,

" MARK BEAUFOY."

VI. *Queries respecting Tanning.*

(To Dr. Thomson.)

" SIR,

" There are few arts which have undergone so little improvement as the art of tanning leather. While science has promoted the interests of other arts, it seems that this art has been either neglected by it, or that the prejudice of those who are engaged in it has been so deeply rooted that they have not chosen to listen to the voice of experimental philosophy. Whatever have been the results of modern experiments, it will, I think, be allowed, that they have not been communicated in a form sufficiently plain to be applied to practice by the tanner himself. You will excuse, therefore, the liberty I take in addressing you on this subject; hoping through the medium of your periodical work to obtain some useful information on the subject of my inquiry. Is it possible to apply Mr. Hatchett's artificial tannin to the purposes of tanning leather? If so, would not the expense of the charcoal and nitric acid be as great, if not greater, than the oak bark now in common use? Does not the use of the lime water injure the subsequent action of the tannin? I am aware that the pigeon's dung acts as a lixivium, and helps to render the lighter skins flexible; but how is it that this lixivium acts upon the skin? Some of these questions, you will say, have been repeatedly asked before; but I have never been able to obtain any satisfactory answers. Fully convinced, as I am, that it is one of your main studies to render science subservient to the interests and the business of common life, I trust that you will not think me impertinent in laying before you the above questions. Hoping to obtain some information on the subject in your *Annals of Philosophy,*

" I remain yours respectfully,

" DEVANUS."

The art of tanning has not been so much overlooked by men of science as my correspondent supposes. Not to mention Seguin, who realized an immense fortune by his fortunate discovery of tannin at the commencement of the French revolution, a society of friends of mine in Glasgow expended five or six thousand pounds

in experiments on the subject, and came at last to this mortifying conclusion, that all the old processes were better than the new, and that all attempts to shorten the time injured the quality of the leather.

I suspect myself that what is called *tannin* by chemists is not a substance uniformly the same, like sugar or gum; but that various bodies may be applied to the same purpose. Mr. Hatchett's artificial tannin is by far too expensive for the purposes of the tanner.

I have inserted the letter of my correspondent in hopes that some of my readers, better acquainted with tanning than I can pretend to be, will give a satisfactory answer to his queries.

VII. *Use of Cow Dung in Calico-Printing.*

While lately visiting some calico printing manufactories in Manchester, I was much struck with one of their processes, the use of which has never been explained in a satisfactory manner. After the mordant has been applied to the cloth, it is necessary to pass the cloth through an infusion of cow-dung in water before it be introduced into the dyeing vat. If this be neglected, the colour is always bad, and is apt to spread unequally on the cloth. Nothing else has been found to answer the purpose but cow-dung; though many other things have been tried; and calico printers are obliged to keep 20 or 30 cows for no other purpose than to furnish dung. We are in want of an explanation of this process. What purpose does it serve? What peculiar substance does cow-dung contain, that renders it indispensable to calico printers?

VIII. *New Explosion in a Coal-Mine.*

A violent explosion of fire-damp has recently taken place in Hebburn Colliery, about half way between Newcastle and South Shields, by which eleven lives were lost. A more particular account of this dismal catastrophe will be given in a future number of the *Annals of Philosophy.*

IX. *Volcanic Sand from St. Vincent.*

I am indebted for the following curious communication to Mr. Smalling, of Camberwell: —

During the eruption of the volcano in the Island of St. Vincent, on the 1st of May, 1812, the ashes were carried to an immense distance, even as far as Barbadoes. A specimen of this brought to England, and subjected to analysis, was found to contain the following proportions of constituents:—

Oxide of iron	1
Calcareous earth	8
Sand and clay	91
	100

X. *Graduation of Glass Tubes.*

I have received the following query on this subject from an anonymous correspondent:—

" On trying your method of graduating glass measures of capacity, I met with a difficulty against which your directions do not provide, and which renders the previous accuracy of operation useless. It is this:—A measure of mercury in the tube or measure to be graduated will have a more convex surface than any body which the glass is intended hereafter to measure, and the degree of convexity differs for different bores. Hence if we mark the highest point of the surface, the measure will be too large; if the lower, or edge, too small. How then do you determine this matter? The thickness of the tube is one source of uncertainty. I have made several measures of thick tube, with a lamp and bellows; but find the widened part, or rim, is very subject to break off, when cold, of its own accord. Pray can you inform me how the tubes themselves are first made, and where?"

My rule, and I believe the method generally followed, is always to consider the uppermost part of the mercury as the place against which the mark on the tube is to be placed : and if this rule is always adhered to, no sensible error will ever be committed. Thick tubes do not answer so well as those that are finer, because they are apt to break of themselves without any assignable cause; so that if we employ them we run the risk of being under the necessity very frequently of renewing our labour. The glass tubes (which are drawn at all the crystal glass-houses) are not annealed, which no doubt is the reason of their being so brittle. I do not know the reason why this process is neglected with respect to them, though probably there is a good one.

XI. *Electroxide.*

To the same intelligent correspondent I am indebted for the following curious fact:—

" I have lately seen a beautiful *electroxide* on a white-washed wall, produced by lightning dispersing a large copper bell-wire, in passing through a house at Cirencester, about seven o'clock on Thursday evening, July 28. Its length on the wall is $6\frac{1}{2}$ feet nearly, and its breadth about two feet. It resembles the effect produced by passing the shock of a battery through bits of gold leaf pressed between two cards. At the same time, the electric fluid, or stroke of lightning, dispersed 40 or 50 feet of iron bell-wire in the lower rooms, and did considerable injury to the house and furniture."

XII. *Saccharometer.*

The same gentleman has proposed the following query :—

" The saccharometer is an instrument used in common breweries to ascertain the strength of infusions of malt, by their specific gra-

vity. But does the specific gravity truly indicate and foretell the strength of the ale or beer which the wort is to make? Are the constituents of malt always dissolved in the same proportions to each other? If not, I think little dependance is to be placed on this instrument."

The saccharometer indicates correctly the specific gravity of the wort before it begins to ferment. The value of the beer or ale (supposing the process properly conducted) always depends upon this strength. I do not mean to say that the price of the ale is always proportional to the specific gravity of the wort; I know very well that the price is often regulated by fashion, and by the reputation of the brewer; but I have made many experiments on the subject, and think myself warranted in concluding from them that the same substances are always taken up by water from good malt, and that the value of the ale is proportional to the specific gravity of the wort.

It may be worth while to observe that the common saccharometers do not give the true specific gravity of wort. I constructed one about ten years ago which indicates the true specific gravity, and the quantity of solid matter dissolved in the wort. It has long been in common use in Scotland; though I suppose it has never made its way into England.

XIII. *Yeast.*

The same gentleman who put the two preceding queries has added the following:—

" In the summer season it is very difficult, and often impossible, for private families in the country to procure barm or yeast to bake with. Pray is any method known of preserving it from the brewing seasons, for any length of time, as two or three months? A process for this would be of extreme utility."

It has long been customary to dry yeast, and to carry it in sacks from the Low Countries to Paris. It is sometimes sent in the same state from Britain to the West Indies. When thus dried, it has a dark colour, and a smell and taste not unlike cheese, or approaching still nearer to that of fermented gluten. I have kept it myself for several years in that state. When mixed with water, it acquires the properties of fresh yeast; but I did not find in my trials (which were made in a distillery) that it went so far as fresh yeast, and on that account advised it to be discontinued. This dried yeast might certainly be employed in private families when fresh yeast is not to be had; and I take it for granted it can always be purchased in London.

XIV. *Annales de Chimie.*

I have been requested by the editors of this work to insert the following notice in the *Annals of Philosophy*:—

Annales de Chimie; that is, Annals of Chemistry, or a collection of memoirs concerning chemistry, and the arts which depend upon it, particularly pharmacy, by MM. Guyton Morveau, Monge,

Berthollet, Sequin, Vauquelin, Adet, Hassenfratz, C. A. Prieur, Chaptal, Deyeux, Bouillon Lagrange, Descotils, Laugier, Gay-Lussac, and Thenard.

These annals form one of the most esteemed scientific journals in Europe. Established in 1789, by the founders of modern chemistry, they have been very regularly published ever since, and form at present one of the richest collections, for the number and importance of the facts which they contain. The editor, M. Klostermann, not being able to continue the publication, in consequence of the embarrassed state of his finances, the contributors have put it into the hands of M. Crochard, bookseller, Rue de l'Ecole-de-Medecine, No. 3, Paris, who is authorised to receive new subscriptions, and to furnish the subscribers with the regular series of the *Annales* as they appear.

Article XIV.

New Patents.

William Sellars, Kemsey Elms, Worcester, engineer; for a method of spinning and laying of ropes, twine, line, thread, mohair, wool, cotton, and silk, by machinery. June 5, 1814.

George Heyward, Brocknor Ironworks, near Stourbridge, Stafford, ironmonger; for an improved plan or method of turning rolls, and of rolling gun and pistol barrels previous to welding. June 7, 1814.

John Stubbs Jorden, Birmingham, copper sash manufacturer; for an improved method of making the lights, and also other improvements in the construction of horticultural buildings. June 7, 1814.

Thomas Tindall, York, Gentleman; for certain improvements on the steam engine, and also a mode of applying the same to the driving of all sorts of carriages and machinery. June 18, 1814.

Bazil Louis Mertian, Threadneedle-street, London; for a method of extracting or separating jelly or gelatinous matter from substances capable of affording the same, in order that the same may be used in the arts, or for domestic purposes. Communicated to him by a foreigner residing abroad. July 12, 1814.

Joseph Smith, London, plater; for a spring hinge for doors and gates. July 16, 1814.

John Dawson, York-street, Dublin; for certain means of producing or communicating motion in or unto bodies, either wholly or in part surrounded by water or air, or by either of them, by the re-action of suitable apparatus upon the said water, or air, or upon both of them. July 16, 1814.

Article XV.

METEOROLOGICAL TABLE.

1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
7th Mo.										
July 10	S	29·83	29·67	29·750	67	44	55·5	—	·16	☽
11	S W	30·03	29·83	29·930	76	51	63·5	—	·17	
12	N W	30·05	30·02	30·035	74	54	64·0	—	—	
13	N W	30·02	29·77	29·895	70	48	59·0	—		
14	N W	29·77	29·68	29·725	69	48	58·5	—	—	
15	W	29·63	29·62	29·625	74	52	63·0	·36	—	
16	N E	29·87	29·63	29·750	66	50	58·0	—	7	
17	S E	29·87	29·85	29·860	73	48	60·5	—	·14	●
18	S W	29·84	29·83	29·835	75	48	61·5	—	·15	
19	N W	29·83	29·62	29·725	74	57	65·5	—		
20	S W	29·70	29·56	29·630	74	56	65·0	—	2	
21	N W	29·87	29·70	29·785	78	56	67·0	·40	—	
22	N W	30·07	29·87	29·970	69	50	59·5	—		
23	S W	30·15	30·09	30·120	82	52	67·0	·34		
24	S W	30·09	29·83	29·960	78	55	66·5	—		☾
25	S E	—	—	—	84	61	72·5	—		
26	N W	30·05	29·90	29·975	80	60	70·0	—		
27	S E	30·05	29·98	30·015	83	70	76·5	—		
28	S E	29·98	29·89	29·935	91	66	78·5	1·08	—	
29	S W	30·05	29·90	29·975	71	56	63·5	—	—	
30	N W	30·10	30·05	30·075	72	65	68·5	·37		
31	W	30·10	29·98	30·040	79	54	66·5	·12	·17	
8th Mo.										
Aug. 1	N W	30·10	29·98	30·040	80	59	69·5	—		○
2	N W	30·10	30·05	30·075	76	64	70·0	—		
3	S W	30·14	30·05	30·095	79	55	67·0	—	—	
4	S W	30·14	29 98	30·060	77	62	69·5	·73		
5	S W	29·98	29·86	29·920	76	58	67·0	—	—	
6	W	30 05	29·98	30·015	69	57	63·0	—	—	
7	S W	29·98	29·80	29·890	70	60	65·0	·57	·23	
		30·15	29·56	29·918	91	44	65·5	3·97	1·11	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Seventh Month.—10. Rain came on gradually this morning, and continued the whole forenoon, after which appeared the *Cumulus* with *Cirrostratus:* the twilight was luminous, with faint horizontal streaks. 11. Wet afternoon: then *Cirrostratus.* 12. Clear morning : after which different strata of clouds inosculating, followed by a slight shower : a little of the *Cirrocumulus.* 13. *Cumulostratus* through the day, changing at evening to *Cirrostratus:* a strong breeze. 14. Clear morning: then *Cumulostratus*, with a breeze. 15. Various modifications of cloud : the day at length overcast, with one or two very slight showers, and more rain in the night. 16. *Cumulostratus*, a. m.: slight showers, p m. : the wind variable : a *Stratus* at night. 17. Misty morning, after which various clouds, with the wind E. Inosculation followed, and a heavy shower in the evening. An electrical smell was perceived at different intervals to-day. 18. Windy : showers through the day. 19. *Cumulostratus*, after some sunshine. At evening the lighter modifications prevailed, including *Cirrocumulus*, in a turbid sky. 20. Wind S. a. m. hollow and threatening rain. Some showers followed, after which the clouds separated, showing several modifications, distinct, and well formed. 21. Fine morning, with *Cumulus.* Groups of thunder clouds formed p. m. chiefly to the E. : but at sun-set the electrical character gave place to *Cirrostratus* and wind. 22. After a clear morning the *Cumulostratus*, which has so long predominated, with its usual attendant, a strong breeze of wind. 23. A nearly serene day. 24. Clear day : a breeze from S. E. 27. Some lightning at night. 28. Frequent vivid lightning in the evening. 29. Some lightning this morning, with rain—a strong breeze from the S.W. all day.

Eighth Month.—3. A few slight showers in the evening. 7. Day showery, with brisk wind.

RESULTS.

Prevailing Winds : Westerly.

Barometer : Greatest height . . . . .30·15 inches;
Least . . . . .29·56
Mean of the period . . . . . .29·918

Thermometer : Greatest height . . . . .91°
Least 44
Mean of the period . . . . . .65·5

Evaporation, 3·97 inches.   Rain, 1·11 inch.

For the latter half of the observations in this period I am indebted to my friend John Gibson.

TOTTENHAM,   L. HOWARD.
Eighth Month, 20 1814.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1814.

ARTICLE I.

*Biographical Account of Mr. Ekeberg, Assistant Professor of Chemistry at Upsala.**

ANDREW GUSTAVUS EKEBERG was born at Stockholm, on the 16th January, 1767. His father, Joseph Erik Ekeberg, was a ship-builder in the service of the King, and afterwards an overseer of the workmen. His mother, Margaret El. Ekorn, was the daughter of a Mr. Ekorn, a manufacturer of morocco leather in Stockholm. At the age of ten he was sent to Calmar, and in 1779 he was boarded in the clergyman's house of Söderokra. Here he acquired the first relish for Greek literature, which during the whole of his life, almost till his last moments, constituted his favourite species of reading. While here, he lost some part of his hearing, in consequence of a violent cold with which he was seized; a calamity that remained attached to him during the remainder of his life, as was very evident when any person entered into conversation with him.

In 1781 he went to the school of Westervik. But next year he accompanied his father, who was led by his employment to go to Carlscrona; and he afterwards returned with him to the capital. In all these situations he continued his studies with the greatest activity, and made uncommon progress both in the sciences and in the art of drawing, for which he had a natural turn. In 1784 he was sent by his uncle, Bruks Director (*Director of Manufactures*) Ekeberg, to the University of Upsala. Here he was chiefly occupied with the mathematics, which constituted one of his most favourite pursuits. In consequence of his genius, industry, and excellent character, he was particularly patronized by Lostbom, Pro-

* Translated from the Kongl. Vetenskaps Academiens Handlingar under Sednare Hälften af in 1813, p. 276.

fessor of Œconomics, and was enabled in consequence to attain the different academical honours with great facility. Accordingly, in 1787 he held a disputation under Professor Thunberg, and on the 16th of June, 1788, received his degree.

In 1789 he undertook a journey to Germany, from Griefswald to Berlin, and back again to Upsala, for which he received a salary. In 1790 he gave a public specimen of his poetical talents, by publishing a poetical discourse on the peace concluded between Russia and Sweden. And in 1792 he translated the dignified lamentation of the Academy, upon the loss of their great benefactor, Gustavus III.

From his proficiency in gaining knowledge, he was induced to add chemistry and analysis to his other acquirements; and in 1794 he gave a specimen of his skill in that art, the substance of which he sent to the Academy of Sciences. In consequence of this display of knowledge, he was appointed Teacher of Chemistry at Upsala. He gave an additional proof of his chemical knowledge in 1796, on which occasion he first employed Klaproth's much admired method of analyzing hard stones. Being well acquainted with natural history, and its different divisions, he gave a specimen of his capability of improving it : in consequence of which, he was recommended to the situation of Adjunct in Practical Œconomics, then vacant; but he did not succeed in procuring the place.

At last, after giving many other proofs of knowledge, after making various mineralogical journeys in Sweden during the summer vacations, and after giving several public courses of chemical lectures, he was in 1799 appointed Assistant Professor of Chemistry, and Operator in the Laboratory.

In consequence of an unfortunate accident, which occurred in 1801, he lost the sight of one of his eyes, and never recovered it. A flask, filled with a detonating mixture of gases, burst in his hand, and a portion of the glass struck the ball of the eye, near the pupil.

In 1797 he published a very good analysis of Gadolinite, a mineral found in Sweden, which contains a peculiar earth first detected by Gadolin, to which the name of *yttria* has been given. To Ekeberg we are indebted for the first accurate description of the properties of this earth. He made experiments likewise on Swedish titanite, and published an account of various minerals containing *tantalum*, a metal which he first discovered ; though it was afterwards ascertained to be the same with the *columbium* of Mr. Hatchett.

On the 4th of June, 1799, he was made an Associate of the Royal Academy of Sciences at Stockholm ; and eleven years afterwards he became a Member of the Royal Society of Upsala.

Mr. Ekeberg was of small stature. He was afflicted with hæmorrhoidal complaints from his infancy. He had likewise a tendency to consumption, which gradually increasing, accompanied by extreme debility, at last deprived him of his life on the 11th of February, 1813.

With a character lively and active by nature, he possessed a

suavity of disposition which threw a glow of kindness over his countenance. But during the latter part of his life his face was marked by a languor, or melancholy, sufficiently conspicuous. He was susceptible of friendship, and sincerely devoted to his friends. But his learning and science constituted the only solace of his life ; for he was burdened with a large family, and had but very slender means of providing for them. The following is a catalogue of all his writings :—

Published separately.

1. Dissertatio Mus. Nat. Acad.   Ups. Pars III. 1787.
2. ————— de Oleis Seminum expressis.   Ups. 1788.
3. ————— de Calce Phosphorata.   Ups. 1793.
4. ————— de Topazio.   Ups. 1796.
5. ————— de Materiis Oleosis e Regno Animali.   Ups. 1796.
6. ————— de nova Analysi Aquarum Mediviensium (in concert with Dr. Berzelius).   Ups. 1800.

In the Memoirs of the Royal Academy of Sciences.

7. Experiments on the black Stone from Ytterby, and on the new Earth which it contains.   1797.

8. Elucidation of the peculiar Properties of *Yttria*, and particularly a Comparison between it and Glucina : with an Account of the Minerals in which this new Earth is found, and a Description of a new Body of a metallic Nature (Tantalum).   1802.

9. Chemical Analysis of a Swedish Titanite.   1803.

In the Journal of Swedish Literature.   1795.

10. Account of the present State of Chemical Science; with many Observations.

11. Poem on the Peace of 1790.   Besides this, he published various other Poems.

ARTICLE II.

On the Composition of Arragonite.

THE striking difference between arragonite and calcareous spar has been long known to mineralogists. The two minerals are distinguished from each other by their hardness, their specific gravity, and the forms of their crystals, which cannot be considered as derived from the same primitive form. Bernhardi indeed has endeavoured to reconcile the crystalline forms of the two minerals with each other. But we have the authority of Haüy for saying that the attempt has not been successful. To what is this difference of properties to be ascribed? Many analyses of arragonite have been

made, in order to detect in it some constituent which does not exist in calcareous spar. But the attempts of Klaproth, Vauquelin, Fourcroy, Bucholz, Chenevix, Thenard, and Biot, were all unsuccessful ; and these skilful analysts were obliged to conclude that its constituents are the very same with those of calcareous spar. About a year ago, a new analysis of arragonite, by Mr. Holme, was read to the Linnæan Society. He found, as had indeed been observed before, that it contained nearly 1 per cent. of water. He therefore considered it as a hydrate, and ascribed the peculiar properties of arragonite to the water which it contained. In a former number of the *Annals of Philosophy*, I mentioned that Stromeyer, Professor of Chemistry at Gottingen, had announced the existence of strontian as a constituent of arragonite. I shall now lay before the English reader a translation of three papers, which are all (as far as I know) that have been hitherto published in Germany on the subject. The first two are by Stromeyer himself; the last is by Gehlen, who repeated and verified the results of the former chemist. These papers will put it in the power of British chemists to determine how far the solution of the problem by Stromeyer may be depended on. If any doubts exist, nothing is more easy than to put them to the test of experiment.—T.

I.

Discovery of the true Nature of Arragonite, and the Chemical Difference between it and Calcareous Spar; in a Letter from Stromeyer, Professor of Chemistry in Gottingen, to Professor Gilbert.[*]

<div align="right">Gottingen, Feb. 28, 1813.</div>

Among various minerals, which I this winter subjected to analysis, is the *arragonite*. It may seem surprising that I should undertake a new analysis of this mineral, which has been already examined by Klaproth, Vauquelin, Fourcroy, Bucholz, Thenard, and Biot. These chemists considered it as pure carbonate of lime, and as differing from calcareous spar neither in the nature nor proportion of its constituents. But however precise and conclusive these analyses may appear, I must acknowledge that I entertained some doubts respecting their accuracy. The structure of arragonite differs so essentially from that of calcareous spar, that the two minerals cannot be referred to the same species. And this is the only case in which chemical analysis and the laws of crystallization are at variance with each other.

It gives me therefore much satisfaction to be able to state, that I have at last ascertained an essential difference between the composition of arragonite and calcareous spar, which puts an end to the apparent discrepancy between analysis and the laws of crystallization. Arragonite, besides carbonate of lime, contains also *carbonate of strontian*, chemically combined with the former in a con-

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stant proportion. It must therefore be considered as a triple compound of carbonic acid with lime and strontian. The proportion in which carbonate of strontian exists in the arragonite is between 3 and 4 per cent.

That so considerable a proportion of carbonate of strontian in this mineral could have been overlooked by those who hitherto examined it, must I think be ascribed to this circumstance, that they conceived sulphate of strontian to be as insoluble in water as sulphate of barytes ; and therefore supposed, that if arragonite contained strontian, that earth would be precipitated from the muriate or nitrate by means of sulphuric acid, or a sulphate, when so much diluted with water that the lime would remain in solution. But that this supposition is not accurate, I have already had an opportunity of showing, in my analysis of sulphate of strontian from Suntel.

Besides, strontian and lime have many common chemical properties. Hence there is some difficulty in separating them from each other ; and I only succeeded in my object by dissolving arragonite in pure nitric acid, evaporating the solution till it crystallizes, and digesting the crystals in alcohol, which does not dissolve the nitrate of strontian.

It is easy to satisfy oneself that arragonite contains strontian, by evaporating a solution of it in nitric acid. When it is sufficiently concentrated, crystals of nitrate of strontian form in the cold solution; and even sometimes during the evaporation, when the solution is very neutral, small octahedral crystals of nitrate of strontian fall down, which remain undissolved when washed in alcohol.

But is this admixture of carbonate of strontian sufficient to account for the striking difference in structure between arragonite and calcareous spar ? This is a question of some intricacy ; but I conceive it must be answered in the affirmative. Various experiments which I have made on native specimens of bitterspar* appear to me to prove decisively, that a small quantity of one substance, mixed with a great quantity of another, may notwithstanding determine the figure of its crystals. Thus, I have analysed perfect rhombs of bitterspar, from St. Gothardt, which contained only 7 per cent. of carbonate of lime. It seemed likewise very evident, from the analysis of various specimens of sparry ironstone, or *steelstone*, as it is called, that their rhomboidal form is owing to the carbonate of lime which they contain. Is it not therefore probable that the crystalline form of arragonite is owing to the carbonate of strontian mixed with the carbonate of lime ? As the carbonate of strontian has not hitherto been found fully crystallized, its structure is unknown ; so that we are unable to recognize it in arragonite.

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* This set of experiments was undertaken in order to examine the law given by Berzelius in the Annalen der Physik, xl. 305, for the formation of triple salts (or double salts, as he calls them). The results which I obtained accord perfectly with the views of that philosopher, and serve to confirm them.

But is not the fact, that arragonite and strontianite have been often confounded together, in favour of my supposition?

I shall conclude by remarking, that I have found some per cents. of carbonate of lime both in the Saxon strontianite, from Braunsdorf, near Freiberg, and in the Scottish strontianite, which Pelletier and Klaproth analysed. It was this mixture of a little carbonate of lime with the carbonate of strontian which led me to the analysis of arragonite. The result has been the discovery which I have the happiness to communicate in the present letter.

II.

On Arragonite, and the Chemical Difference between it and Cal-careous Spar. By Stromeyer, Professor of Chemistry at Got-tingen.*

The history of the author's examination of this problematic body is as follows:—As he was engaged last winter in the analysis of a mineral discovered at Braunsdorf, near Freiberg, which some consi-dered as a strontianite, and others as an arragonite, it occurred to him that it would be worth while to subject arragonite itself to a new chemical analysis. He found the mineral in question a strontianite, containing some per cents. of carbonate of lime. On analysing again the Scottish strontianite, examined by Klaproth and Pelletier, he found that it likewise contained some per cents. of carbonate of lime. Hence he was led to suspect that some of the native carbo-nates of lime might also contain some carbonate of strontian. The great external resemblance between arragonite and strontianite led him to pitch upon that mineral as most likely to verify his sus-picion.

Kirwan had already conjectured, from the similarity between ar-ragonite and strontianite, that the former contained a portion of strontian. But Bucholz and Thenard, who examined arragonite expressly to determine whether it contained any strontian, could not detect in it the smallest traces of that earth. But their experi-ments did not appear to the author sufficiently decisive to demon-strate the absence of strontian in arragonite. Besides, various analyses of bitterspar, and of the mineral called *steelstone,* had satisfied him that the acute notion of Professor Haussman, respect-ing the influence of the power of crystallization of certain sub-stances in determining the crystalline form of other substances, was not without foundation; and that substances which possess a very strong tendency to crystallization, even when mixed in very small proportions with other bodies, may yet be able to impress upon them their own determinate crystalline form. It was therefore not very unlikely that the crystalline form of arragonite might de-pend upon the presence of strontian.

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* Gilbert's Annalen der Physik, xlv. 217. October, 1813.

Perfectly neutral nitrate of strontian is insoluble in alcohol; while, on the other hand, nitrate of lime dissolves in alcohol. Upon this property the author grounded his method of examining whether arragonite contained any strontian.

Even the first trials which he made answered his expectation. He employed, for the purpose, arragonite from Vertaison, in Auvergne, with which almost all preceding chemical analyses had been made. On the cooling of a neutral and sufficiently concentrated solution of arragonite in nitric acid, and even sometimes during the evaporation, some octahedral crystals separated which were insoluble in alcohol, and possessed all the properties of nitrate of strontian. More than twenty experiments, made with perfectly pure, and with various varieties of crystallized arragonite, gave the very same result; so that there could be no doubt that arragonite contains some per cents. of strontian.

These experiments were repeated with the prismatic arragonite from Migranilla in Valentia and from Molina in Arragon, with the columnar arragonite from Dax in the *ci-devant* Bearn, from Iberge in the Hartz, from Neumarkt in the Oberpfalz, and with the columnar and fibrous arragonite from Mordklinge in Lowenstein in Swabia, and from the Faroe Islands. All these yielded the same results as the arragonite of Auvergne. A portion of nitrate of strontian was extracted from each.

In two minerals to which the term arragonite has been applied, namely, *ironbloom (eisenbluthe),* and *fibrous lime (föserkalke)* from Westphalia, no strontian was found. The ironbloom is a pure carbonate of lime, and in the fibrous lime there are some per cents. of gypsum. But neither of these minerals possesses the character of arragonite, while they both agree perfectly with the properties of rhomboidal calcareous spar.

Experiments, which the author made with a great variety of specimens of calcareous spar, showed that carbonate of strontian is an essential constituent of arragonite: for in none of them did he find the least trace of strontian. In two specimens of columnar calcareous spar, indeed, when he treated the dry nitrate of lime with alcohol, a very slight muddiness took place, which was removed by the addition of a few drops of water, and was not again produced by the muriate of barytes. Hence it might be owing to the presence of an atom of strontian. But both these specimens of calcareous spar exhibited here and there traces of a conchoidal fracture.

After the author had convinced himself that arragonite differs from calcareous spar, by containing as a constituent a portion of carbonate of strontian, and that arragonite is in reality a triple salt, he undertook the exact chemical analysis of three of the most remarkable varieties of arragonite; namely, that from Bearn, that from Molina in Arragon, and that from Auvergne. And by way of comparison with them, he analysed two very pure and perfectly transparent specimens of calcareous spar; one from Iceland, the

other from the Hartz. The result was, that the proportion of carbonate of strontian varies in different varieties of arragonite; but that it is constant in the same variety. He has found the same rule to hold with respect to the magnesia in bitterspar, dolomite, and *bitterkalk.* The Arragonese and Bearne arragonite contains twice as much strontian as that of Auvergne; while this last contains one and a half times as much as the varieties from the Hartz and from Faröe.

The author found (fully verifying his opinion respecting arragonite) less carbonic acid in arragonite than in calcareous spar, in whatever way that quantity was determined; whether by exposing the minerals to a strong heat, or by expelling the carbonic acid by means of an acid, or by the quantity of carbonate of lime formed by passing the gas, extricated, through lime water.

There exists another striking difference between arragonite and calcareous spar. Arragonite contains a small quantity of water chemically combined, as is obvious from the appearance of an enamel, and the efflorescence which takes place when arragonite is exposed to a slight heat. Rhomboidal calcareous spar contains no water chemically combined with it; and though exposed to the heat at which arragonite is altered, still retains its lustre, its transparency, and all its properties. It begins to alter only when the carbonic acid makes its escape. The small quantity of water driven off by heat from some varieties of calcareous spar is very various, and is only mechanically contained in them. Hence those spars only give water which decrepitate in water; and they yield the more the stronger the decrepitation is. This, as all decrepitations, is owing to the escape of water or air, which is mechanically confined between the lamellæ of the crystals, and therefore ought not to be considered as characteristic of certain bodies. The remarkable transparency of Iceland crystal is owing, I conceive, to the total absence of water in it: for it does not decrepitate, and it may be heated almost to redness without undergoing any change. The efflorescence of arragonite, when exposed to a moderate heat, gives us, as Haüy has already remarked, an easy and sure mode of distinguishing it from rhomboidal calcareous spar.

The author found likewise evident traces of oxide of manganese and oxide of iron in arragonite. The first is in all probability in the state of a carbonate, and chemically combined with the other two carbonates. The arragonite of Auvergne contains none of it. Hence it is not an essential ingredient. The oxide of iron seems to be in the state of an hydrate, and only to be mechanically mixed between the lamellæ of the mineral. This appears obvious from the Spanish arragonite, which contains the most oxide of iron, and owes its colour to it; for when put into acids it becomes colourless and transparent; while at the same time the hydrate of iron separates from the plates, and subsides. This hydrate contains a mixture of quartz, sand, and of gypsum, neither of which is to be considered as a constituent of arragonite.

The following table exhibits the constituents of 100 parts of arragonite, according to the medium of a number of experiments, varying but little from each other :—

Constituents.	Columnar from Bastene and Dax, in the deparment of Landes.	Prismatic from Molina, in Arragon.	Columnar from Vertaison, in Auvergne.
Carbonate of lime	94·8249	94·5757	97·7227
Carbonate of strontian ...	4·0836	3·9662	2·0552
Protoxide of manganese with a trace of iron..	0·0939	—	
Hydrate of iron, with quartz, sand, and gypsum	—	0·7060	—
Hydrate of iron	—	—	0·0098
Water of crystallization..	0·9831	0·3000	0·2104
	99·9855	99·5489	99·9981
Or,			
Lime	53·3864	53·6225	55·0178
Strontian	2·8808	2·8187	1·4498
+	0·0939	—	0·0098
Carbonic acid	42·8669	42·4476	43·2896
Water of crystallization..	0·9831	0·3021	0·2104
	100·2111	99·1909	99·9774

The following table exhibits the result of the analysis of the two specimens of pure rhomboidal spar :—

	From Iceland.	From Andreasberg.
Lime	56·15	55·9802
Oxide of manganese, with a trace of iron......................	0·15	0·3563
Carbonic acid	43·70	43·5635
Water of decrepitation	—	0·1000
	100·00	100·0000

These proportions of native carbonate of lime agree with the latest determinations of Berzelius and the author respecting artificial carbonate:* and afford a new proof that natural combinations consist of the same invariable proportions as the artificial.

* Carbonate of lime, according to Berzelius, is composed of 56·4 of lime and 43·6 carbonic acid.

III.

On Strontian as a Constituent of Arragon.   By the Academician Gehlen.*

(In a Letter to the Editor, dated March 4, 1814.)

According to your wish, I communicate to you, for insertion in your Journal, my experiments on Stromeyer's discovery of strontian in arragonite; and I do it the more readily, because there is some difficulty in bringing this constituent into view.

Professor Stromeyer's discovery became known to me at Landshut. The effect which it produced on Professor Fuchs and me induced us to undertake a set of experiments in order to assure ourselves of the accuracy of the statement.

But though we followed the mode pointed out by Mr. Stromeyer, we were not able to obtain any satisfactory results respecting the strontian. We digested alcohol of about 90° upon the dry solution both of Spanish and French arragonite. There remained no ponderable residuum, though the experiment had been made on 200 grains of arragonite. A trace of undissolved matter remained, which was not taken up by water. Both solutions were gently evaporated to the consistence of a syrup, and set aside in that state; but no crystals of nitrate of strontian made their appearance in the course of a week, as we had expected them to do.

Another method was now attempted; namely, the evaporation of a solution of arragonite in nitric acid previously mixed with a certain proportion of solution of gypsum. It was expected that a double decomposition would take place, and that the strontian would in this manner be separated from the great proportion of lime with which it was mixed, and might in consequence be subjected to farther examination. A previous experiment with a solution of 192 grains of carbonate of lime mixed with eight grains of carbonate of strontian verified this expected conclusion. This solution being mixed with the solution of gypsum, and evaporated to the consistence of a syrup, let fall fine soft needles. These being separated by the filter, and washed with weak alcohol, coloured the flame of the blow-pipe purple red (as is the case wth sulphate of strontian). It was now treated with an alkaline carbonate, and the residuum, being washed, was dissolved in muriatic acid. The solution crystallized in fine needles, soluble in alcohol, and giving that liquid the property of burning with a purple colour, and thereby showing decisively that it was strontian.

But a similar experiment made with a solution of arragonite was not equally successful; for the portion that separated was not of such a nature that it could be readily recognised. My departure for Vienna prevented the farther prosecution of these experiments.

* Schweigger's Neues Journal für Chemie und Physik, x. 133. 1814.

We were anxious to try the effect of the absolute alcohol of Richter. Mr. Moser, in Vienna, to whom I stated what I had done, gratified my wishes, and made the experiment with absolute alcohol. With a solution of 96 grains of carbonate of lime and four grains of carbonate of strontian, the alcohol left a residuum which exhibited the properties of nitrate of strontian : but 100 grains of Spanish arragonite treated in this way gave so little insoluble matter, that it could not be distinguished upon a small filter. Mr. Moser wished to prosecute these experiments.

After my return, in January of this year, a copy of Bucholz's Pocket Book for 1814 came to hand. Here (p. 32—48,) he relates his unsuccessful attempts to obtain strontian from arragonite, either by the process of Stromeyer, or by one which he himself had contrived; namely, to decompose the dry residuum, obtained by evaporating the solution to dryness, by means of a red heat, and to separate the caustic strontian from the lime by means of its greater solubility in water. When to all this I add that a celebrated French analyst, to whom Stromeyer's discovery had been communicated by a German residing in Paris, was equally unable to succeed in verifying it, I do not see any reason that I had to be surprised at my own want of success.

Yet the fact was so simple ; the experiments made by Stromeyer depended upon properties sufficiently established; and they required nothing but attention to ensure accuracy. I could not bring myself to conclude that Stromeyer, of whose usual accuracy I was well aware, had repeated his experiments a great number of times without determining the nature of the matter separated, notwithstanding the facility with which that could be done. Various circumstances occurred to me, which might have prevented my success, and rendered a repetition of the experiments requisite, with all the precautions suggested by our previous knowledge. I had the good fortune at last to be successful, and to confirm the statement of Stromeyer. At the meeting of the Mathematico-physical Class of the Royal Bavarian Academy of Sciences, on the 28th of March, I gave an account of these experiments, and laid before them the strontian contained in arragonite, separated by Stromeyer's and Bucholz's methods, and in combination with nitric acid ; first, in the state of crystals ; and secondly, dissolved in weak alcohol ; exhibiting its combustion with the well known red flame, and contrasting it with the yellow flame yielded by nitrate of lime.

I shall now state a few particulars respecting the experiments.— After I had prepared absolute alcohol, I in the first place made a comparative experiment with a direct mixture. A solution of 384 grains of carbonate of lime and 16 grains of carbonate of strontian in nitric acid was divided into two equal portions. The one half was evaporated to dryness in a small porcelain capsule, and the residuum being reduced to a fine powder in the same temperature, and the whole of the water which it contained being driven off, it was covered with a glass plate, and allowed to cool. It was then

brought to the consistence of a syrup with a portion of absolute alcohol. This was thrown into a glass, and the capsule was washed clean with alcohol. The glass was then kept agitated, and absolute alcohol was gradually poured in till the powder was completely dissolved. The solution thus formed was milky, and in 12 hours deposited a white sediment, and became clear. The whole was thrown upon a filter kept covered with a glass plate, and what remained upon the filter was washed with alcohol. It exhibited the properties of nitrate of strontian.

The other half of the solution was (in order to verify the accuracy of the process contrived by Bucholz, for his own trials were upon a very small scale,) evaporated to dryness, and the dry mass exposed to a red heat in a platinum crucible till the whole nitric acid was decomposed. The residue was digested in boiling hot water in a covered platinum crucible, the milky liquid was thrown upon a covered filter, and what remained upon the filter was likewise washed with hot water. The whole of this liquid solution, which might amount to rather more than six ounces, being put into a retort furnished with a receiver, was distilled down to the quantity of $1\frac{1}{2}$ drams. Next morning small sandy crystals were visible upon the sides and bottom of the retort. Unexpectedly there appeared, half an hour after, a distinct, though small, groupe of plumose crystals, obviously of strontian. Without waiting any longer, some nitric acid was poured into the retort, and the solution was evaporated to dryness. Even during the evaporation small crystals separated, which, being kept, were washed with absolute alcohol in the same manner as nitrate of lime, described above.

Two solutions of columnar arragonite from Auvergne, each of 200 grains, were treated exactly in the same manner. In these experiments the strontian did not crystallize so soon as in the preceding ; but it showed itself in the same manner when the retort was washed with nitric acid, and when the solution was treated in the way before described.

Thus is the discovery of Stromeyer confirmed ; and thereby a dispute, long carried on, finally settled. It shows us that between two branches of the same science not a single discrepancy exists ; and that when any such makes its appearance, it is not necessary for us to modify the principles of either the one branch or the other ; for we may be certain that the discrepancy will finally disappear by the discovery of some ingredient hitherto overlooked. In respect of chemistry, in particular, it shows us that we must have recourse to all the aids with which the science furnishes us : and that though the analysis has sometimes to struggle with great difficulties, it does not finally forsake us.

There still exists a similar discrepancy between chemistry and oryctography, in respect of the anatase and rutil ; but these minerals, unless I deceive myself, contain the titanium which is common to both in different states of oxidation ; and there is the same difference between them as exists between those ores of iron which

contain the iron in a minimum and maximum state of oxidizement.

As to what Stromeyer has said in the above quoted letter to Professor Gilbert respecting the influence of the strontian which it contains upon the crystallization of arragonite so different from the form of calcareous spar, I might assent to it from what I myself said in your journal in my dissertation on prehnite, &c. (vol. iii. p. 198;) but the opinion being still undecided, in consequence of our ignorance of the crystalline form of carbonate of strontian, I may put the question, whether, when bases and acids are crystallized, either the combination of the same base with different acids, or of different bases with the same acid, they do not follow the same law, according to which, when the figure of certain compounds of the same base is known, the still unknown figures of the other compounds may be conjectured? It were to be wished that our crystallographers were more of crystallologists than they have hitherto been.

It is of some importance to inquire into the cause why Stromeyer's method of separating strontian from arragonite failed at first, both in my hands, and in those of various dexterous and accurate chemists. I am not yet qualified to give an opinion on this subject, as I have not yet seen Stromeyer's paper on the subject, and therefore do not know the minute details of his experiments. From the columnar arragonite of Auvergne I succeeded in separating strontian by means of alcohol by both processes; and I sent the matter remaining on the filter to Bucholz, for his farther satisfaction. With the arragonite of Neumarkt, on the contrary, from which I easily obtained strontian by Bucholz's method, by saturating the solution obtained from the residue after exposure to a red heat, with nitric acid, and evaporating, though not in so large a quantity as from the French arragonite, the treatment with alcohol does not seem to answer so well. The solution indeed was opalescent at first; but it did not become muddy till after an interval of some days, and at last precipitated a very fine slime, by no means in considerable quantity. Perhaps it is better after the evaporation of the solution to dryness, which separates any excess of acid that may be present, to dissolve the residuum in as much warm water as will dispose the solution to crystallize on cooling, and then to treat the crystallized mass with alcohol, according to Stromeyer's method. That Bucholz did not succeed in the method contrived by him might be partly owing to the earthen vessel in which he decomposed the nitrate by a red heat; for when such a vessel is employed, it is obvious that a considerable portion of the matter must be lost; but his failure seems to have been principally owing to his having trusted entirely to the crystallization of the strontian, obtained by water from the residue exposed to a red heat, without employing any other test of its presence.

The most striking circumstance of all is, that Biot and Thenard, whose apparatus for measuring and weighing was considered as very complete, found the same proportion of carbonic acid in arragonite

and carbonate of lime. This determination must be very doubtful, if we consider the different proportion of carbonic acid in carbonate of lime and carbonate of strontian, and recollect that Stromeyer has found 3—4 per cent. of carbonate of strontian in arragonite.

I might write to you here of several other observations which I made during the course of my experiments, especially of a remarkable appearance which I observed during the decomposition of the nitrates by a red heat; but as I could not make you acquainted with the conclusion of my experiments, I think it better to defer the whole to a future letter, satisfying myself at present with having confirmed and established Stromeyer's discovery.

ARTICLE III.

On the Analogy between the Prismatic Spectrum and the Musical Division of Sound. By David Huston, Esq.

(To Dr. Thomson.)

SIR,

THE remarkable analogy subsisting between the diatonic division of *sound* and the prismatic division of *light* has been often observed, and has given rise to the following observations, and perhaps sweeping conclusions :—

1. That the *minor* mode in music is the most natural ; and that the prismatric spectrum exhibits that mode or key, if Newton's primitive colours be regarded as 1st, 3d, and 5th, or the common chord.

2. That the rays of heat accompany those of light in a spectrum * of two series, in the adjoining key (to speak musically) to that of light : that is to say, the " fifth" of the prismatic series (blue) is adopted by the spectrum of heat for its fundamental or key-note— descending and increasing in intensity till it reaches the third (being there *without* the red ray or key-note of the visible spectrum) above the diapason ; from whence it diminishes in the same ratio as the coloured rays, until it has completed the octave. That the rays termed *deoxidizing* are only the more refrangible rays of *heat;* and that they climb or extend beyond the violet ray up to the key-note (or situation of blue, supposing another series of prismatic colours) *exclusive.*

3. That if we imagine *a series of prismatic spectra* in succession, like the octaves of a piano-forte, we shall ever find that metals oxidate *downwards:* in other words—that as their dose of oxygen increases, the colours they exhibit move down in this order ; tin, antimony, and arsenic, being exceptions, but not violations of this law.

* I have used this word from sheer necessity.

Reasons.

1. The accompanying figure, No. I., is Newton's measurement of the spectrum divided into 12; No. II., the minor series of sound exemplified by the blank chromatic divisions; No. III., the major series exhibited in a similar manner.

No. I.
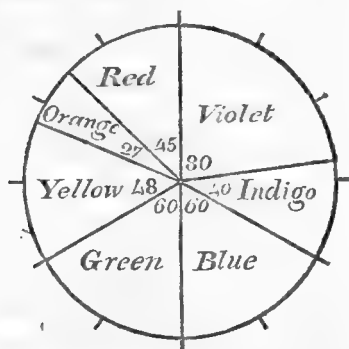
No. II.—*The Minor Series.*

No. III.—*The Major Series.*

It may be readily seen by comparison of measurement, that if yellow be considered a third to the key, that the key is minor; for, if we choose a great third, we must adopt the *commencement* of green. And here it may be necessary to insist on what must be known to every person latently, though seldom, I believe, prominently considered—namely, that every sound except the one we set off from is only an announcement of distance from that one (as a clock informs us of the number of hours which are *past*), and not a signification of any quantity between two points, but merely a very point itself. A contrary notion is apt to be imbibed by those in the practice of using keyed instruments.

It is worthy of remark that persons who have little opportunity of hearing artificial music almost always adopt the *minor* mode when they attempt to sing: the wild chanting of a country boy is invariably minor, and, which is still more singular, so are nearly all the celebrated London Cries.

2. The reasons for this hypothesis arise of course from the celebrated experiments of Dr. Herschel and Sir H. Englefield; but particularly from the latter gentleman's statement of the degrees of heat exhibited by the thermometer placed in the several prismatic rays from *blue* downwards. As the intensity of light in the spectrum is found in the region analogous to the situation of the musical third; so, if we imagine the rays of heat to be in a kindred key with light—to bear the same relation that the key of E minor bears to the key of A minor in music—we may immediately see a reason why the greatest heating power is *without* the range of the prismatic colours; for there is the situation of the third of E minor. It is not so easy to account why the deoxidating power should be greatest beyond the violet ray. I am inclined to think (from analogy only, for I have not the means of making experiments) that the commencement of the violet colour must be the seat of its greatest energy; and if this is not the case, I can only suppose that this faculty increases with the refrangibility.

3. All the metals that I am acquainted with (gold and copper excepted) display when most devoid of oxygen a pale blue colour— or that point in the spectrum where heat is first acknowledged by the thermometer. By a reference to your valuable Table of Metallic Oxides, it will be seen that they obey the order just pointed out in a very decided manner; that as they lose their heat and acquire weight by the addition of oxygen, so they descend in the order of colours (supposing spectrum to follow spectrum),—not in the same gradations; for some appear to take surprising leaps, while others glide through almost every shade. Lead is an example of the latter; and steel (respecting the *oxidizement* of which Sir H. Davy has lately satisfied himself) in the tempering fairly and cleanly passes through and completes the octave:—I say octave, because the last colour is light blue, from whence it started; and all this before it becomes the *black* oxide, the first stage:—to make the peroxide, it has to descend again to *red* (repeating, perhaps, all that progress which we were before enabled to view, and forming the distance of what is called a *twelfth* in music from the black oxide). Copper has been observed also to display a variety of tints before the black oxide is confirmed: I doubt not but that they proceed in the same line of march. Gold presents us with a purple oxide.

Hoping I have rendered myself intelligible, but much fearing that I have not,

I am, Sir, yours most respectfully,
DAVID HUSTON.

Article IV.

On the Antilunar Tide.　By John Campbell, Esq. of Carbrook.

(To Dr. Thomson.)

DEAR SIR,

I observe in your Number for June last a notice relative to the antilunar tide; and although your Correspondent has not entered into the subject in such a manner as to require an answer from either the advocates for the Newtonian theory, or from me; yet, as it affords an opportunity of illustrating a point in which I humbly conceive the opinions I have published in your work have a considerable advantage over those taught in the Newtonian School, I shall offer a few remarks on the article.

Your Correspondent W., though he does not seem to perceive it, has given you a crude edition of Ferguson's theory. The principles on which he accounts for the antilunar tide are,—1. That the farther parts of a fluid body (by which he means the parts near the surface) have greater centrifugal than centripetal force, and would fly off, were they not restrained by the general mass; whilst the near parts (*i. e.* nearer the centre) have more centripetal than centrifugal force, and would fall to the centre, were they not restrained by the same cause. 2. That for these reasons a fluid body turning round a centre out of itself must assume the form of an oblong spheroid, gravity and projectile force being the sufficient and obvious cause of each; that is, he ascribes the antilunar tide to an excess of centrifugal force. According to Mr. Ferguson, the antilunar tide is caused by the centrifugal force of the side of the earth farthest from the moon being increased by the enlargement of the circle round the centre of the orbit in which the earth is supposed by him to move. One can understand Mr. Ferguson's principle. An enlarged orbit travelled in equal time must increase velocity and centrifugal force. The only unfortunate circumstance in his explanation is, that it is inconsistent with the fact. The side of the earth farthest from the moon does not always perform the largest orbit, and therefore his ingenious theory cannot be true.

This insuperable objection evidently occurs with equal force to the opinion of W., who, like Mr. Ferguson, introduces, as the cause of the antilunar tide, the excess of centrifugal force in the farthest part of the earth, moving round a centre of itself. For unless the movement of the antilunar side of the earth be encurvated, round a centre out of itself, the circumstances, which in his opinion obviously cause the tide, do not exist. But, as has been stated in opposition to Mr. Ferguson, the antilunar side of the earth does not always move in a curve concave to the moon; and therefore the centrifugal force, as introduced by W., will not apply. As little will the centripetal force avail him; for even were there any foun-

dation for his notions as to the nearer parts having an excess of gravity to the centre of the orbit, the tide thereby occasioned would be a solar tide, and would correspond to the forces of the attraction of the sun, and not of the moon. In truth, however, there is no foundation for the opinion of your Correspondent, as to the excess of centrifugal and centripetal forces in the farther and nearer parts; and therefore I need not pursue the argument against them. What I wish chiefly to submit to your consideration, and through your work to those who will admit of no improvement on the discoveries of Newton, is the circumstance, that the Newtonian theory of the antilunar tide, as well as that opposed to it by Mr. Ferguson, requires certain movements of the earth, which withdraw the phenomenon from being the simple effect of gravitation.

This, though not conclusive, appears to my mind a strong objection to the Newtonian theory. The lunar tide is admitted on all hands to be a simple effect of the moon's attraction; but when we inquire as to the cause of the opposite tide, another principle is introduced in aid of what on the nearer hemisphere is of itself sufficient. Every analogy pleads against this: besides, if the moon's attraction is strong enough to produce such an effect on one side of the earth, what arrests its progress, what sets the bound to its operation, on the opposite side? It must operate with a force diminished merely by the increased distance; and therefore any other additional principle would, as I have elsewhere stated, produce a redundant effect.

I beg leave to put a question, to which I hope for a reply. Suppose the miracle of Gideon were to be repeated on a more extensive scale; that the voice which bade the planets roll were to fix them stationary in any points of their orbit; in such circumstances, would or would not the antilunar tide cease to flow? The lunar tide, it will not be disputed, would continue to oblongate the earth. Would there be no corresponding swell on the opposite hemisphere? Let the Newtonian explain how this would happen on his principles; I need not recall to your recollection the simplicity with which it is explained on mine.

I am, dear Sir, yours faithfully,
JOHN CAMPBELL.

ARTICLE V.

On the Rise of Water in the Chesswater Mine.
By Mr. John B. Longmire.

[To Dr. Thomson.]

SIR,

I READ in a late Number of your valuable journal some questions by Mr. Moyle, of Helston, in Cornwall, respecting the entrance of

water into mines.   Mr. Moyle mentions an influx of water into the
Chesswater mine, which he endeavoured to account for; but on his
ideas not meeting with the approbation of his friends, he was in-
duced to request you, or some of your readers, to " explain the
cause on proper hydrostatic principles."   The following observa-
tions were made in consequence of this request, and are respectfully
submitted to yours, his, and his friends' consideration.

Mr. Moyle says that the Chesswater mine is 120 fathoms deep.
At the bottom of this mine the water is collected into a level of
considerable length, and is then lifted out of it, by a steam-engine,
to the earth's surface.   Lately, after the engine had ceased working
a few minutes, and when the lowest level was full of water, a feed
of water very unexpectedly made its appearance at the top of a
small pit, *or wins*, one fathom deep, which is situated in a level 16
fathoms above the lowest level, and then passed along the former
level to the engine-shaft, in which the water " rose slowly and pro-
gressively."   Mr. Moyle, because " water will always find its own
level, let what will retard its progress," was justly surprised at its
entrance into the mine at such a height, while the reservoir below
remained in a great measure empty; but he thinks the following
reasoning will account for it.   The places where the water entered
the mine at the lower level are just sufficient to admit the regular
feeds ; but after the water had covered these inlets, and in propor-
tion to the height to which it rose, it offered such a resistance to the
feed of water, that a part of it rose to the next outlet, the head of
the wins, while the remainder entered the mine, as usual, at the
lowest level.

The altitude of the feeds of water in the channels through which
they pass to enter the lower level, would certainly be increased
when that level was filled with water; because the resistance which
the water in it would offer to the entry of the feeds would be equal
to the friction that the feeds would meet with in passing through
the water in the level, and up to its surface, in the pit.   But it is
evident that the channels would be *completely filled* with water to
the top of the wins before any part of the feeds, which before en-
tered the lower level, could pass along the upper level to the pit ;
and, in consequence, the water in these channels would posses a
power equal to that of a column of water of 16 fathoms high, to
force itself into the lower level.   Now this is a power adequate to
overcome the resistance it would meet with in passing through *a
very long horizontal passage* filled with water ; but whether or not
it would be more than sufficient to force itself to the engine-shaft
is what I cannot answer with certainty, because the distance is not
exactly ascertained ; but, as Mr. Moyle says the lower level is " se-
veral fathoms " in length, I suppose the feeds of water are not
situated from the shaft above 50 or 60 fathoms; and if so, the
increased altitude of the water to 16 fathoms, and the consequent
entry of a part of it into the upper level, cannot, I think, be pro-
duced by the resistance that the feeds meet with in travelling to the

engine-shaft; because the resistance is made a cause *much less* powerful than the effect it is supposed to produce.

Two or three probable conjectures may be offered to account for the sudden appearance of the water in the upper level; but no person, except one that is intimately acquainted with the *peculiarities* of that part of the mine in question, can give the real cause. If the water be a part of that which before entered the lower level, we may suppose that at the time the engine ceased to work, the channels through which it entered the lower level were too small for the *then* quantity of water in the feeds; so that they could not force themselves into it, without first completely filling the channels to within two or three fathoms of the higher level, and thereby acting with the power equal to that of a column of water of the same height and area. With this altitude we will suppose the feeds *could* make their way into the lower level, as they were collected into the channels. But when the engine stopped, and the water had filled the lower level, they would have to overcome the resistance which the water in this level opposed to them in their progress through it: to overcome this resistance a certain increase of altitude was necessary; but before this was acquired, the feeds reached the upper level, and a part of them, if not the whole, passed along this level to the shaft. It is possible that the appearance of the water in the upper level may be owing to some temporary stoppage, either in the lower level, between where the feeds enter this level and the engine-shaft, or in the channels. If either of the two last instances be correct, the appearance of the water so high in the mine will be of short duration. It is also possible that either the stoppage may be permanent, or the feed of water in the upper level may not have communication with those in the lower level : in either case the water will continue to run along the former level. But of these conjectures I think the first is the most probable.

I am, Sir, with much respect, your humble servant,

J. B. LONGMIRE.

Kendal, in Westmoreland,
July 31, 1814.

ARTICLE VI.

Experiments to determine the Constituents of Azote.
By John Miers, Esq.

(*Concluded from* p. 193.)

Symbolic Representation of the intire Changes that have taken place in the Gases now under Consideration.

WATER, H O, and sulphureted hydrogen, S H, contaminated with an uncertain portion of hydrogen, H, as is always the case

when it is procured from the sulphuret of iron, were passed through a red-hot copper tube.

, We will suppose $\overset{4}{H}\overset{4}{O} + \overset{4}{S}\overset{4}{H} + \overset{5}{H}$ passed on; one S is deposited either in combination with the copper, or separated and sublimed on the colder part of the tube. $\overset{3}{O}$ S form one sulphuric acid that is absorbed by the water. $\overset{1}{O}$ combines with $\overset{6}{H}$ to form $\overset{1}{A}$, which enters into combination with $\overset{2}{S}$ and $\overset{3}{H}$. $\overset{4}{H}$ are left in an uncombined state. Now the gases received over water are found to consist of 4 per cent. oxygen, 52 hydrogen, and 44 unknown gas, possessing similar properties to azote. These gaseous products will be found to agree with the foregoing deduction, the small portion of oxygen being considered as accidental, or as derived from the water. The symbol of the unknown gas, then, is $A\overset{2}{S}\overset{3}{H}$.

The $\overset{4}{H}\overset{4}{O} + \overset{4}{S}\overset{4}{H} + \overset{5}{H}$ are thus resolved : S and $O\overset{3}{S}$ are condensed, while $A\overset{2}{S}\overset{3}{H}$ and $\overset{4}{H}$ are received over water in a permanent state.

These gases, in consequence of some internal change of affinities, become condensed in time to nearly half their volume. Small regular crystals deposited themselves on the sides of the vessel, and, at the periods of opening the stop-cock, atmospheric air rushed in to supply the vacuum produced by the condensation of the contents. The gas became changed in its properties. It had acquired a peculiar fetid smell; and although so large a portion of oxygen was admitted in the atmospheric air, it seemed all absorbed in its new combination, as not one particle was to be discovered by any of the known modes of detecting its presence. Notwithstanding also that a very large portion of hydrogen had originally existed in the gaseous product, none was now discoverable in it, but by some chemical change, which let free a part from its combination. A peculiar acid gas was now found to exist in the gas, possessing very peculiar properties. This acid gas existed in a state of combination with the hydrogen, which it threw off whenever it entered into new combinations. A portion of the azote of the atmospheric air, admitted into the flask, was also found to have entered into the composition of the acid gas. The quantity of atmospheric air admitted into the flask is to be represented by $\overset{4}{A}\overset{2}{O}$ (two atoms), $\overset{1}{S}$ having been previously deposited in the crystals.

Now, on examination, the *mixed gas* in the flask is found to consist of about 48·7 acid gas, 15·3 hydrogen, and 36 azote, per cent. Hence we may ascertain the state of this change: $\overset{4}{A}\overset{2}{O} + A\overset{2}{S}\overset{3}{H} + \overset{4}{H} - S$ are resolved into $(\overset{2}{S}\overset{2}{O}\overset{4}{A}\overset{3}{H} + \overset{3}{H}) + A$. The

symbol of an atom of acid gas is, then, $S \overset{2}{O} \overset{2}{A} \overset{4}{H}$; of the hydrogen combined with it, $\overset{3}{H}$; of the azote, residuum $\overset{3}{A}$.

On the Composition of the Unknown Gas, resulting from the Decomposition of Water.

According to the views afforded by the foregoing calculation, its atom will be expressed by the symbol $A \overset{2}{S} \overset{3}{H}$. We will see how this result agrees with its ascertained properties, and we can at the same time deduce its composition. It will be recollected that 100 cubic inches of this gas weighed 28·02 grains (p. 185). The weight of its atom will be $(4 + 1\cdot799 + 0\cdot399 =)$ $6\cdot198$.

Thus for	As the weight of a whole atom	Is to its individual compound	So is the quantity of gas obtained	To the proportion of its constituents.	And so is the weight of 100 cub. in.	To the weight of its constituents,	And to the volume of its compound gases.
Sulphur	6·198 :	4	:: 4·556 :	2·941	:: 28·02 :	18·09	=
Azote	6·198 :	1·799	:: 4·556 :	1·322	:: 28·02 :	8·13	= 27·5
Hydrogen	6·198 :	0·399	:: 4·556 :	0·293	:: 28·02 :	1·8	= 80·71
	6·198		4·556			28·02 =	108·21

It is here shown that such a combination of azote and hydrogen gases holding sulphur in solution would weigh near 28 grains for every 100 cubic inches. It will appear singular when we observe that the proportion of the azote to the hydrogen is exactly similar to that of ammonia : in short, that this gas is a combination of the elements of one atom of ammonia united to two atoms of sulphur, and that such a compound exists in the gaseous state. Could it be supposed as a sulphuret of ammonia, our surprise would increase when our attention was turned to its chemical properties; alkaline sulphurets are not only easily soluble in water, but are always eager to decompose it; in this case we perceive no relation whatever between it and water. The present æra is remarkable for the extreme peculiarity of its newly discovered bodies; and we should not suffer the singularity of such a compound to lead us into the belief that it cannot exist. The combination is doubtless to be effected by other processes, different from the one in which this was procured.

To ascertain the Weight of an Atom of Acid Gas,

I passed 2·25 cubic inches of the gas extracted from the flask through a solution of the super-acetate of lead ; a precipitate of small black shining flakes instantly formed, which, when collected

and dried, weighed 1·72 grain. Now the mixed gas has been elsewhere shown to contain 48·7 per cent. in volume of acid gas. Hence if 100 : 48·7 :: 2·25 : 1·09575 = volume of acid gas combined with the oxide of lead :—100 cubic inches of acid gas have also been shown to weigh 35·38 grains. Hence if 100 : 35·38 :: 1·09575 : 0·3876 = weight of acid gas combined with the oxide of lead—and 1·72 − 0·3876 = 1·3323 = weight of the oxide of lead combined with the acid gas. The precipitate, then, consists of

Yellow oxide of lead	1·3323
Acid gas	0·3876
	1·72

We may hence calculate the weight of an atom of acid gas to be as—

Oxide of lead.		Acid gas.		Atom of oxide of lead.		Atom of acid gas.
1·3323	:	0·3876	::	27·974	:	7·9591

Calculation of the Composition of the Acid Gas from the Weight of its Atom.

We have elsewhere deduced the symbol of an atom of acid gas to be S^2 O^2 A H^4 : the weight of its atom must therefore be 2 + 2 + 3·599 + 0·533 = 8·133. This will be found to agree wonderfully with that deduced from its combination with the oxide of lead, 7·959, the numbers being as near as the nature of the experiments could lead us to expect. We may therefore proceed to determine the proportions of the constituents of the acid gas.

8·133	:	2	::	100	:	24·6	Sulphur.
8·133	:	2	::	100	:	24·6	Oxygen.
8·133	:	3·599	::	100	:	44·255	Azote.
8·133	:	0·533	::	100	:	6·545	Hydrogen.
8·133				100			

Calculation of the Constituents of the Acid Gas from Experiment.

We will attempt to deduce the composition of the acid gas from its combination with the oxide of lead. It is most probable that an atom of acid gas is combined with an atom of oxide of lead. We may hence proceed :

For Sulphur.—The lead must therefore be combined with sulphur in the precipitate in half the proportion to that in which it exists in the common sulphuret. Now the yellow oxide is composed of 100 lead and 7·71 oxygen ; the sulphuret, of 100 lead and 15·42 sulphur. Then as 107·71 : 100 :: 1·3323 : 1·237 = quantity of lead in the precipitate—and as $100 : \frac{15·42}{2} :: 1·237 :$ 0·0953 = quantity of sulphur in the precipitate.

Hence if—

Acid gas.		Sulphur.		Acid gas.		Sulphur.
0·3876	:	0·0953	::	100	:	24·6

For Oxygen.—The lead must be combined in the proportion of one atom to two atoms of oxygen, the same combination as the yellow oxide of that metal. Then as

L		O		L in precipitate.		O in precipitate.
100	:	7·71	::	1·237	:	0·09535

A G   O   A G   O
And 0·3876 : 0·0953 :: 100 : 24·6 oxygen per cent.

For Azote.—We are not acquainted with any combination of azote with lead; but we have seen that there exists an atom of azote for every atom of oxygen in the precipitate. Hence we may arrive at the quantity of azote in the precipitate. As 1 : 1·799 :: 0·0953 : 0·17155 azote in precipitate—and as 1 : 1·799 :: 24·6 : 44·255 azote per cent. in acid gas.

For Hydrogen.—We are also unacquainted with any combination of lead with hydrogen; but we have calculated that there exists two atoms of hydrogen for every atom of oxygen in the acid gas. We may hence deduce the quantity of hydrogen in the precipitate. As 1 : 0·266 :: 0·0953 : 0·0253 = hydrogen in precipitate—and as 1 : 0·266 :: 24·6 : 6·54 = hydrogen per cent. in acid gas. The constituents of the acid gas, then, are found to exist in these proportions :—

	In the precipitate.		In the quantity formed in flask.		Per cent.
Sulphur	0·09535	=	1·5993	=	24·6
Oxygen	0·09535	=	1·5993	=	24·6
Azote	0·17155	=	2·8757	=	44·26
Hydrogen . . .	0·02535	=	0·4262	=	6·545
	0·3876		6·5005		100

We will proceed to inquire how these numbers agree with each other, according to the laws of combination observed in most cases.

The oxygen should be combined with lead in the precipitate in a similar or multiple quantity of that in the oxide of lead. The quantity of yellow oxide of lead in the precipitate is 1·3323 ; and as 107·7 : 7·7 :: 1·3323 : 0·0953 oxygen in the yellow oxide of lead in the precipitate, a quantity exactly corresponding to that of the acid gas existing in combination with it. Hence the law of Berzelius, that the oxygen of the acid is a proportional quantity to that of the oxygen of the base, holds here exactly. According to the same laws, the azote ought to contain an equal or proportional

quantity of oxygen to that ascertained above. Now we have found that there exists 0·1715 gr. azote in the acid gas of the precipitate.

Hence if $\overset{A}{100} : \overset{O}{55\cdot6} :: \overset{A}{0\cdot1715} : \overset{O}{0\cdot09535}$ = oxygen in the azote, being exactly equal to the weight of the oxygen in the oxide of lead.

The quantity of sulphur in the acid gas of the precipitate has been shown to be 0·0953 ; and it has been deduced to exist in combination with the lead in half the proportion to that of the common sulphuret. Now there exist 1·237 gr. of lead in the precipitate ; and to convert this into a sulphuret would require 0·1907 gr. of sulphur. If we take half this quantity, 0·09535, we shall have the exact proportion deduced to exist in the acid gas of the precipitate.

The hydrogen has been shown to exist in combination with the oxygen of the acid gas in a double proportion to that existing in water. Then as $\overset{O}{1} : \overset{H}{0\cdot133} :: \overset{O}{0\cdot09535} :: \overset{H}{0\cdot0126} \times 2 = \overset{H}{0\cdot0253}$ in the acid gas of the precipitate.

Exposition of the Arrangement of the Elements during the Changes under Consideration.

The mixed gas extracted from the flask had for its constituents 4·556 unknown gas + 0·437 hydrogen + 0·507 oxygen = 5·5 grains, originally occupying its capacity 37·704 cubic inches; and 5·7275 azote + 1·5225 oxygen = 7·25 atmospheric air = 12·75 grains, that now filled the same capacity. It therefore consists of

	Weighing grains.	Equal in volume to cub. in.
Unknown gas	4·556	16·259
Hydrogen	0·437	19·618
Oxygen	2·0295	6·026
Azote	5·7275	19·375
	12·75	61·278

There was a deposition of solid materials from these, calculated to consist of

	Grains.	Cub. In.
Sulphur	1·3417	
Azote	0·1598	0·546
Oxygen	0·4302	0·277
Hydrogen	0·0683	3·062
	2	4·885

10·75 gr. = 56·393 cubic inches

Condensed by their peculiar combination into the capacity of 37·704, or about 150 volumes into 100, which will be seen to agree with that ascertained by calculation, as described in the general table that will shortly follow.

Now the mixed gas is found to consist of

	In weight grains.	Equal in volume to cub. in.	Existing in a condensed state of cub. in.
Sulphur	1·5993 =		=
Azote	2·8757	9·728	5·308
Oxygen	1·5993	4·749	2·5982
Hydrogen	0·4262	19·103	10·4557
Acid gas	6·5005	33·58	18·3619
Hydrogen in combination } with the acid gas ...... }	0·2355	10·56	5·7687
Azote remaining	4·014	13·5734	13·5734
	10·75	57·7134	37·704

The volumes of the gases in their present state will therefore be seen to agree with those of the materials from which they were formed : we will now proceed to inquire how the respective quantities of materials have individually been disposed of.

The following gaseous products were formed from the elements of water, sulphureted hydrogen, and hydrogen:—

S	A	O	H		
2·941	1·322	—	0·293	= 4·556	Unknown gas.
—	—	0·507	—	= 0·507	Oxygen.
—	—	—	0·437	= 0·437	Hydrogen.
				5·5	Weight of gas in flask, April, 1812.
—	5·7275	1·5225	—	= 7·25	Atmospheric air admitted Nov. 17, 1813.
2·941	7·0495	2·0295	0·73	=12·75	Weight of materials.
1·3417	0·1598	0·4302	0·0683	= 2	Weight of crystals and moisture deposited on the flask.
1·5993	6·8897	1·5993	0·6617	=10·75	Weight of mixed gas.

The new arrangement of these elements will exist under the following form:—

S	A	O	H		
1·5993	2·8757	1·5993	0·4262	= 6·5005	Acid gas.
—	—	—	0·2355	= 0·2355	Hydrogen combined with it.
—	4·014	—	—	= 4·014	Azote uncombined.
1·5993	6·8897	1·5993	0·6617	=10·75	Weight of mixed gas.

By considering an atom of *acid gas* to weigh 8·133, we have before calculated that it consists per cent. by weight of 24·6 sulphur, 44·26 azote, 24·6 oxygen, and 6·54 hydrogen.

The *mixed gas* is found to consist of

	Cub. In.		Cub. In.	Grs.
Hydrogen	.28 condensed into		15·3 weighing	0·6244
Azote	.36 occupying		36	10·6416
Acid Gas			48·7	17·234
			100·	28·5

Hence the weight of 100 cub. in. of acid gas is found to be 35·38 grs.

From these data we may calculate the volumes of the different constituents of the *mixed gas*, and the general view of its constitution will be seen in the following table :—

	ACID GAS. 100 Cubic Inches consist			HYDROGENATED ACID GAS. 100 Cubic Inches consist			MIXED GAS. 100 Cubic Inches consist			Capacity of Flask consists		
	By Weight of Grains.	Of Volumes of Gases occupying in a Natural State. Cub. In.	Condensed State. Cub. In.	By Weight of Grains.	Natural State. Cub. In.	Condensed State. Cub. In.	By Weight of Grains.	Natural State. Cub. In.	Condensed State. Cub. In.	By Weight of Grains.	Natural State. Cub. In.	Condensed State. Cub. In.
Sulphur ...100 : 24·6 :: 35·38	8·7047	—	—	6·623	—	—	4·24	—	—	1·5993	—	—
Azote ....100 : 44·26 :: 35·38	15·651	52·8078	28·908	11·911	40·29	21·997	7·624	25·717	14·078	2·8757	9·728	5·308
Oxygen...100 : 24·6 :: 35·38	8·7047	25·8514	14·15	6·623	19·66	10·767	4·24	12·588	6·891	1·5993	4·749	2·5982
Hydrogen 100 : 6·54 :: 35·38	2·3196	104·0179	56·942	1·765	79·14	43·328	1·13	50·658	27·731	0·4262	19·103	10·4557
Acid gas	35·38	162·6771	100·	26·922	139·09	76·093	17·234	88·963	48·7	6·5005	38·58	18·3619
Hydrogen combined with the acid gas				0·976	43·77	23·906	0·6244	28·	15·3	0·2355	10·56	5·7687
Hydrogenated acid gas ...... Azote uncombined, in a natural state of expansion ...				27·898	182·86	100·	10·6416	36·	36·	4·014	13·5734	13·5734
Mixed gas							28·5	152·963	100·	10·75	57·7134	37·704

Hence it will appear that 100 cubic inches of *acid gas* consist of 182·677 cubic inches of gaseous matter, holding dissolved 8·7047 grains of sulphur, all condensed into a volume of 100 cubic inches. The weight of all these materials, 35·38 grains, corresponds exactly with the calculated weight of 100 cubic inches of acid gas.

The *hydrogenated acid gas* seems to be a curious combination. The action of the acid on metals and metallic salts appears to differ very materially from that of other known combinations. Although, like muriatic acid gas, it contains hydrogen united to an acid basis; yet in combining with oxides its hydrogen is liberated, while that of muriatic acid gas is retained in combination.* The hydrogen on its liberation is found to occupy a larger volume than when united to the acid gas; and if we were to estimate the quantity of acid gas in the mixed gas from the amount of absorption by a metallic solution, we should evidently be wrong. By adding the difference between the volumes of the disengaged hydrogen in its combined and un-combined states, to the amount of the apparent absorption, the product will yield the volume of the acid gas in the mixed gas. We will cite an example: a portion of the mixed gas was sent through a solution of the muriate of bismuth, when a bronzy precipitate immediately formed from the union of the acid gas with the oxide of bismuth; the amount of diminution was 33·77 per cent. The residuum contained hydrogen, which, on being deto-nated with oxygen in Volta's eudiometer, indicated an amount of 30·46 per cent. of the original volume of the mixed gas. Now the volume of the hydrogen in the condensed state of combination was 16·76. Then 30·46 − 16·76 = 13·7 difference of volumes: 33·77 + 13·7 = 47·47, volume of acid gas per cent. of mixed

$$\overset{\text{M G}}{} \quad \overset{\text{A G}}{} \quad \overset{\text{H}}{}$$

gas. Hence 100 − 47·47 − 16·76 = 35·77 azote remaining, which quantities will, on referring to the table, agree remarkably well with the calculated volumes of the various ingredients of the mixed gas. The small increased amount of hydrogen is owing to the gas having remained a long while over mercury, the results of the table having been estimated from the data afforded by its action on the oxide of lead when first put to trial.

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* This is owing to the constitution of muriatic acid. It will perhaps be worth while to mention that I am engaged in an interesting pursuit, in endeavouring to discover the form, disposition, and constitution, of the ultimate particles of bodies; and the conclusions at which I am arrived are highly satisfactory. One of these conclusions enable me to announce that the long contested dispute with regard to the nature of chlorine will probably be shortly settled. I have deduced an atom of dry muriatic acid to be composed of three atoms of hydrogen and three atoms of oxygen, or of 11·75 hydrogen and 88·25 oxygen; chlorine, of one atom of muriatic acid and one atom of oxygen, or 3 H + 4 O, or 90·93 hydrogen and 9·07 oxygen, or 77·26 muriatic acid and 22·73 oxygen; muriatic acid gas, of one atom of chlorine and one atom of hydrogen, or one of muriatic acid and one of water, or of 88·25 oxygen and 11·75 hydrogen, or 97·07 chlorine and 2·93 hydrogen, or 75 muriatic acid and 25 water. The weight of an atom of muriatic acid will then be 3·399; of chlorine, 4·399; of muriatic acid gas, 4·533; the two latter being about equal to those ascertained by Sir H. Davy.

Having proceeded thus far, we will now show how the results of experiment correspond with those ascertained by calculation in the foregoing table.

It will be remembered that experiment indicated the presence of 42·42 per cent. of condensed hydrogen in the mixed gas. It has been calculated that there exists 27·731 in the acid gas, and 15·3 combined with it = 43·031—a quantity agreeing with the above as well as the uncertainty of the data could lead us to expect.

The results of the action of the mixed gas on various solutions exhibit proofs of its constitution according to the foregoing calculations:—

Acetate of iron absorbed 34·78 per cent.; hydrogen in the residuum amounting per cent. of the mixed gas to 29·89. Hence the mixed gas is composed of

Acid gas .... 48·25
Hydrogen .... 16·42
Azote .... 35·33
                                   ——————
                                   100·

Muriate of bismuth absorbed per cent., 33·77; hydrogen liberated, 30·46:—

Acid gas .... 47·47
Hydrogen .... 16·76
Azote .... 35·77

Prussiate of mercury absorbed per cent., 56·86; hydrogen liberated, 13·17:—

Acid gas .... 62·79
Hydrogen .... 7·24
Azote .... 29·97

The cause of the great difference of result with this prussiate depends no doubt on some mutual change with the prussic acid that deserves examination.

Water absorbed per cent., 39; hydrogen liberated, 27·56:—

Acid gas .... 51·475
Hydrogen .... 15·085
Azote .... 33·44

If a small portion of the hydrogen and azote were also absorbed by water (as is very probable), the results will then come close to our calculated proportions.

Phosphorus condensed per cent., 11·11. The residuum had a portion that precipitated metallic solutions equal to 35·55 per cent. of mixed gas. The remainder contained hydrogen per cent. of the gas, 30·98:—

Acid gas .... 49·58
Hydrogen .... 16·95
Azote .... 33·47

Potash absorbed per cent., 31·16 ; hydrogen liberated, 36·46 :—

Acid gas	47·56
Hydrogen	20·06
Azote	32·38

At present I refrain from entering farther upon this subject ; it rests with the cultivators of chemistry to satisfy themselves of the correctness of these views, and to prove or disprove the facts contained in the foregoing pages. For this reason I forbear applying any name either to the " unknown gas " or to the " acid gas " till the truth of their existence is confirmed beyond my own assertion. In the present stage of the investigation I think I may be warranted in drawing the following

General Conclusions.

1. It seems sufficiently well established that azote is a compound body, and that it has been formed from the elements of water.

2. There can exist but little doubt that azote is composed of 55·6 oxygen and 44·4 hydrogen per cent. in weight.

3. It is evident that such a compound as sulphureted azote exists, and that it is composed of 52·65 sulphur and 47·35 azote, or of one atom of sulphur and one atom of azote.

4. The new gas described under the title of the " unknown gas " is one that has never before come under the notice of chemists. It would appear to be composed of two atoms of sulphur united to the elements of one atom of ammonia. It is very similar in its want of chemical properties to azote.

5. The crystals deposited on the sides of the flask appear also novel combinations. Their form is certainly different from that of sulphur ; and their composition, as deduced in a former place, is only analogous to some of those of carbon.

6. The acid gas is likewise a new and singular compound ; its properties being decidedly different from those of any other known substance. It is a combination analogous to the prussic acid ; the former being composed of azote, hydrogen, and oxygen, united to sulphur ; while the latter is formed of the same materials united to carbon. Several striking coincidences might be pointed out in the relation of these two compounds.

I am now on the point of recommencing my experiments on the composition of azote, the obstructions to their progress being now, I hope, entirely removed. Whenever my little leisure will allow of their being completed, I will not fail to give you full information of their results.

I remain, Sir, with much respect,
Your obedient humble servant,

London, June 28, 1814.   JOHN MIERS.

ARTICLE VII.

Memoir on Palladium and Rhodium.   By M. Vauquelin.

IV.

Properties of the neutral Ammonio-muriate of Palladium, or of Submuriate of Palladium.

This salt has a very fine rose colour. In this point of view palladium is better entitled to the name of rhodium than the metal which bears it.   When we examine this salt we find that it is composed of very delicate, flexible, and brilliant needles, which form a spongy mass, very soft to the touch.

Reduction.—Twenty grammes (308·88 grains) of the dry salt heated strongly in the heat of a forge in an earthen crucible furnished eight grammes (123½ grains), or 40 per cent. of a metal having the colour of unpolished silver. Its parts were united together, but incompletely melted.

This metal might be hammered or passed between rollers without breaking.

To see better what takes place during the decomposition of this salt of palladium by heat, I exposed it to the blow-pipe. It melted, diminished much in volume, and exhaled vapours of sal ammoniac and oxymuriatic acid.   It is the melting and the diminution of volume of this salt which, bringing the particles of metal nearer each other, renders them susceptible of being hammered and rolled out into plates.

Solubility.—The red salt of palladium is very little soluble in water. It merely gives it by long contact a slight tint of yellow. It is but little soluble in diluted muriatic acid, when not assisted by heat ; but at a boiling temperature the acid dissolves it in considerable quantity.   The solution is of a yellowish brown colour. When the excess of acid of the solution is saturated with ammonia, the salt precipitates with its rose colour, and all its other properties. If an excess of alkali be added, the liquid assumes a slightly yellowish colour.

If instead of ammonia, potash be employed to saturate the excess of muriatic acid, the salt precipitates in yellow flocks ; but if we add ammonia, they become red, showing that they again unite with ammonia and form the triple salt.

V.

Some Properties of Palladium.

This metal has some resemblance to platinum in its colour, malleability, hardness, and fusibility.

The heat of our furnaces does not melt it completely. I have

succeeded only in fusing it in small quantities upon charcoal, the combustion of which was increased by a jet of oxygen gas.*

I have observed that if the heat be kept up after it is melted, it enters into ebullition, and burns, throwing out very brilliant sparks. A portion of the metal which has escaped the combustion dissipates, and is condensed on the surface of the charcoal in the form of very small grains.

Platinum melted in the same manner does not burn like palladium, which shows that this last metal is more volatile and more combustible. I found the specific gravity of palladium, rolled out into a plate, 12 and a very small fraction.

VI.

Solution of Palladium in Nitro-muriatic Acid.

A gramme (15·444 grains) of palladium placed in contact with six grammes of nitro-muriatic acid, composed of equal parts of the two acids, was speedily attacked without the assistance of heat, and the liquid assumed a reddish brown colour. At first no nitrous gas was disengaged; but when heat was applied, a great deal of this gas made its escape. This shows that palladium requires a great deal of oxygen in order to dissolve.

The solution of palladium is of a reddish brown colour, which is the more intense the more acid it contains; for in proportion as the excess of acid is driven off, the colour diminishes, and a fawn coloured salt is obtained, little soluble in water, and giving that liquid a yellow colour; but the solution becomes reddish brown on the addition of some drops of muriatic acid.

Neutral muriate of palladium is but little soluble in water, but very soluble in water acidulated with muriatic acid. The solution does not crystallize regularly.

Liquid muriate of ammonia put into the acid solution of palladium occasions no precipitate; but if the liquids are concentrated, there speedily form a great number of needle-form crystals, of a greenish yellow colour, and very remarkable in their appearance. When the crystallization takes place slowly, this salt presents the form of four-sided prisms, or elongated hexagons. It is ammonio-muriate of palladium. If to the solution of this triple salt we add some drops of ammonia to saturate the excess of acid, a very brilliant crystalline precipitate falls, of a fine rose colour, which is the sub-muriate of palladium-and-ammonia already mentioned.

The solution of potash gives a brown colour to muriate of palladium, and separates from it red and brilliant flocks, appearing as if composed of plates. The liquid remains coloured, notwithstanding the excess of alkali; but when heat is applied, the whole of the oxide of palladium is thrown down, and the liquid becomes colourless. The alkaline carbonates produce the same effect.

* Mr. Chenevix says that palladium melts in the ordinary heat of our furnaces; but it is probable that what he experimented on was not pure.

The infusion of nutgalls produces no change in the solution of palladium; but when a few drops of ammonia are added to the mixture, a greenish precipitate falls. The liquid retains a yellow colour, which announces the action of ammonia on that combination.

Muriate of tin forms a black precipitate when poured into the solution of palladium. Green sulphate of iron throws it down in the metallic state.

When the oxide of palladium is separated from the acid by means of potash, it retains a great deal of water, at least its volume is very considerable. In this state it has a reddish brown colour. When dried its bulk diminishes greatly, and it acquires a very brilliant black colour.

Muriate of palladium then does not form a triple salt with potash, as is the case with muriate of platinum.

The oxide of palladium, well washed, and dried as much as possible in a gentle heat, loses 20 per cent, and becomes metallic. Hence it contains a great deal of oxygen.

VII.

Sulphuration of Palladium.

A hundred parts of the rose-coloured triple salt of palladium, heated with its own weight of sulphur in a covered crucible, furnished 52 parts of a bluish white sulphuret, very hard, and when broken exhibiting brilliant plates in its fracture.

Knowing from preceding experiments that 100 parts of the salt contain from 40 to 42 of metal, it was easy to determine the quantity of sulphur which had combined in the above experiment. It is evident that if 42 of metal (the proportion which I consider as the most probable) absorb 10 of sulphur, 100 parts of palladium would require 24 of sulphur to convert them into a complete sulphuret.

The sulphuret of palladium put into a cupel melted completely at the temperature at which silver is assayed. When a certain proportion of the sulphur was dissipated, the metal became solid, increased in volume, and became rough on the surface. When the whole sulphur was separated, the palladium was of a silver white colour. It flattened easily under the hammer, and could be passed between rollers without cracking.

Sometimes palladium obtained by burning off the sulphur presents on its surface spots of a greenish blue colour, which seem to be owing to a commencement of oxidation; for they disappear in muriatic acid, while the acid acquires a reddish colour. The same thing happens when, in decomposing the triple salt, heat enough is not given to drive off the oxygen completely. Platinum does not unite in this manner to sulphur.

VIII. Rhodium.

Examination of the Ammonio-muriate of Rhodium.

After having separated the palladium, and having evaporated the liquor containing the muriate of rhodium, to obtain that salt in crystals, the mother water was poured off, and the crystals allowed to drain. They were then repeatedly washed with alcohol, as has been mentioned before. This liquid dissolves the green matter interspersed through the crystals, which acquire in consequence a very fine ruby red colour. It now requires much more water to dissolve it than before it was treated with alcohol. Its solution does not crystallize, as it did at first, because the alcohol has removed the excess of acid, which was very favourable to its solution and crystallization.

The solution of muriate of rhodium freed from the copper, iron, and traces of palladium, which might still remain in it, forms with ammonia a precipitate of a fawn yellow colour, granular, and but little soluble in water. It is a submuriate of rhodium-and-ammonia, as is evident from the vapours of sal ammoniac driven off when the salt is heated.* The liquid from which this submuriate had been separated preserves a pretty deep fawn colour, notwithstanding the excess of ammonia. When heated, without being boiled, it furnishes a new quantity of flocky precipitate of a brighter yellow than what fell at first. When evaporated to dryness, and treated with water, it still leaves a small quantity of a similar precipitate. All these precipitates were submuriate of rhodium-and-ammonia. This shows that the salt is to a certain degree soluble in water, and that an excess of ammonia favours the solution.

Solubility.—Ammonio-muriate of rhodium dissolves very readily in cold water. The solution has a reddish purple colour, similar to that of cochineal, or the fresh juice of currants ; but heat renders the colour brown, and, by keeping, it becomes brown without the application of heat.

This salt is granular, crystalline, and very brilliant. It is not soft to the touch, like the submuriate of palladium-and-ammonia.

IX.

Decomposition of Ammonio-muriate of Rhodium by Potash.

Two grammes (30·88 grains) of the salt dissolved in water being mixed with a solution of potash, a rose-coloured precipitate fell, and ammonia was exhaled. In a short time the quantity of precipitate diminished, and the liquid assumed a greenish yellow colour. When heat was applied, the whole precipitate dissolved, the colour

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* This name agrees with it still better on this account, that the salt furnishes 50 per cent. of metal when heated ; while before being precipitated by ammonia, it gave only 28.

of the liquid became more intense, and much ammonia was disengaged. There was an excess of potash in the liquid.

This solution, left to itself for some days in an open vessel, yielded fawn yellow crystals. They probably consisted of potash-muriate of rhodium dissolved by an excess of alkali. What seems to prove this is that, when the excess of potash is saturated with muriatic acid, a yellowish white precipitate appears, which is but little soluble in water. It is a neutral muriate of rhodium-and-potash.

X.

Reduction of the Ammonio-muriate of Rhodium.

Fifteen grammes of this salt strongly heated in a forge in an earthen crucible yielded 4·2 grammes of a white metal, which had not been melted, but all the parts of which were agglutinated together into one mass. In another experiment ten grammes of the salt yielded nearly three grammes of metal. Hence the salt contains 28 or 29 per cent. of metal.

Desiring to know if this metal could unite to sulphur, and in case it could, how much sulphur was requisite to convert it into a sulphuret, I mixed four grammes of the triple salt with an equal weight of sulphur, and exposed the mixture for a few minutes to the heat of a forge. I obtained a button completely melted, of a bluish white colour, and weighing 1·4 gramme. We see here that the rhodium had combined with sulphur. Hence the complete fusion and the addition of weight, which otherwise would not have been more than 1·12 gramme. Hence the sulphuret of rhodium contains 26 per cent. of sulphur.

The sulphuret of rhodium, exposed to a strong heat in the open air, exhales sulphurous acid, and becomes rough on the surface. After the process the metal is white, spongy, and brittle, and weighs only 1·1 gramme.

Fusibility.—Rhodium appears to be the most infusible of all known metals. Half a gramme of this metal, obtained from the submuriate, decomposed at a moderate heat, and therefore black, was heated for a long time on charcoal, the combustion of which was supported by oxygen gas; yet it was not melted. Its parts only conglutinated into a single mass, having the colour of silver. I have repeated this process several times with still smaller quantities of metal, without being able to melt it completely. This metal then, though brittle, is more difficult to melt than palladium and platinum, which are easily fused by a fire supported by oxygen gas. This infusibility prevented me from determining the specific gravity of the metal.

We may say then that pure rhodium is a metal which has a white colour not much different from that of palladium. It is brittle, and more difficult to melt than any other metal.

XI.

Solubility of Rhodium in Acids.

A gramme of rhodium in fine powder exposed to the action of eight grammes of nitro-muriatic acid composed of equal parts of its two constituents was not sensibly attacked, and the acid scarcely became coloured.

Seeing that this metal is not dissolved in nitro-muriatic acid of a moderate concentration, I treated it anew with a very strong acid ; but it was not dissolved any more than the first time. As the rhodium, when pure, is insoluble in all acids, both simple and compound, we must suppose that in the ore of platinum it is alloyed with other metals that favour its solution, when crude platina is treated with nitro-muriatic acid. It has not been in my power, in consequence of this insolubility, to examine the properties of the simple salts of rhodium.

I shall give, in a second memoir, the method of separating osmium and iridium from the foreign bodies which constitute the insoluble residue in the ore of platinum ; and I shall likewise describe the properties of these metals as far as I have examined them.

Article VIII.

Register of the Weather in Plymouth for the first Six Months of 1814. By James Fox, jun. Esq. With a Plate.

(To Dr. Thomson.)

SIR,

Agreeable to my promise,* I send you a copy of my meteorological journal for the first six months of the year 1814. You will perceive that I have included January in it (though given in my former communication) ; my reason for so doing is, that if you should deem it worthy the expense of an engraving, the table will be more perfect with the addition of that month. It being my intention to continue these observations for three years, six such engravings would make a pleasing map of the atmosphere of Plymouth ; whereas if January be omitted, it will be rendered incomplete.†

I am, &c.

James Fox, jun.

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* *Annals of Philosophy*, vol. iii. p. 274, April, 1814.
† Mr. Fox has stated, in a private letter, reasons for considering the enormous fall of snow in January as measured by him to be correct ; and I must own they appear to me satisfactory.—T.
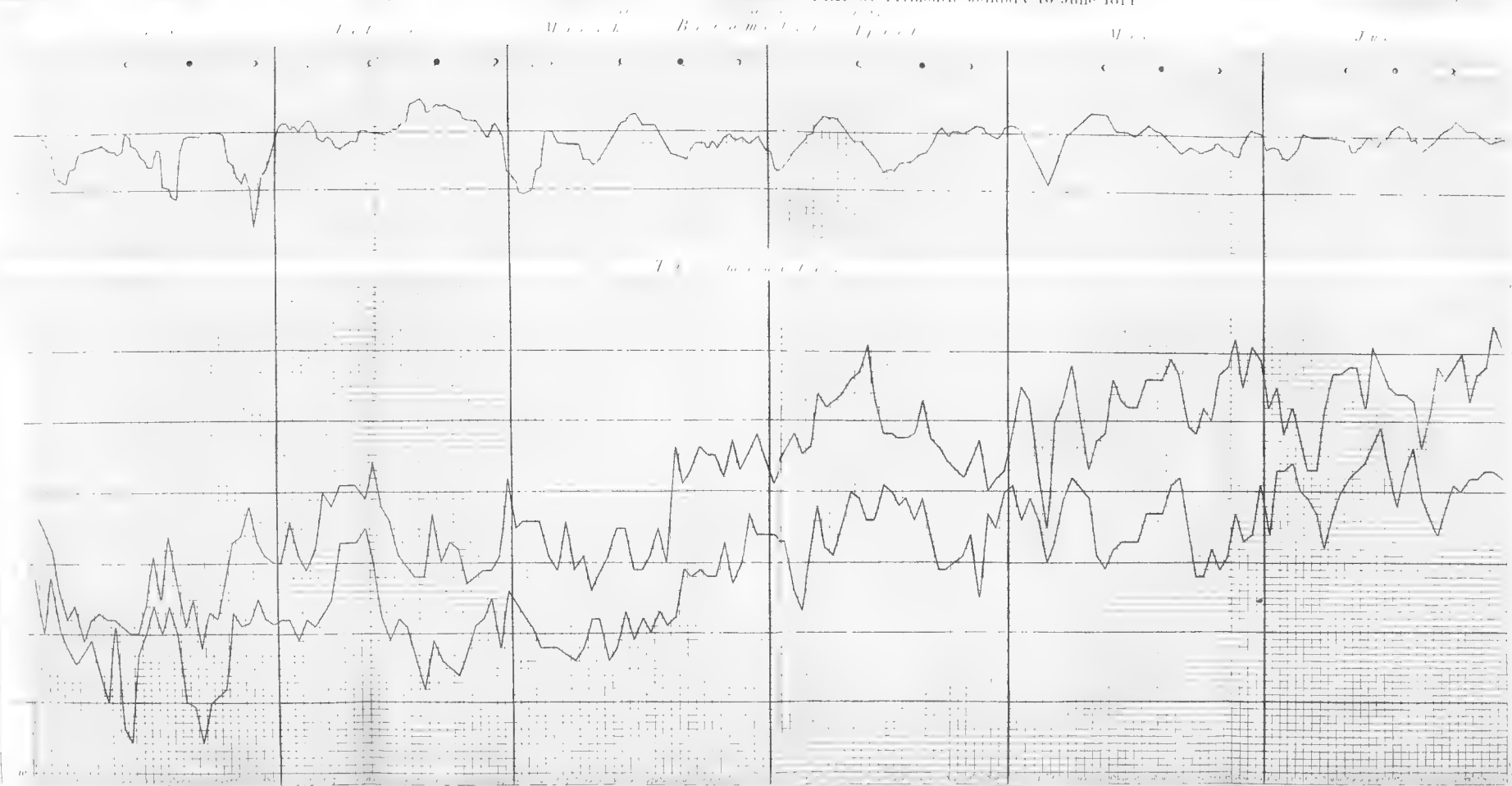
Winds, State of the Weather, and Quantity of Rain, at Plymouth, between January 1 and June 30, 1814.

JANUARY.

Date.	Wind.	Rain.	Observations.
1814.			
Jan. 1	E	—	Hazy morn; bright afternoon.
2	Var.	—	Fog and hoar frost.
3	E	0·6	Showery.
4	E N E		Snow showers (light).
5	N		Ditto, ditto.
6	N E		Ditto, ditto.
7			Cloudy, high wind.
8			Ditto, ditto.
9			Fair.
10	N E to S E	3·2	Ditto, morn; snow, eve.
11	S E		Snow.
12	N		Cloudless day.
13	N E		Fair morn; cloudy eve.
14	E		Sleet.
15		0·3	Cloudy, and showers.
16	N E		Ditto.
17	E		Ditto.
18		3·5	Fog, morn; eve, *very* heavy rain.
19	N E		Morn, *rain;* hail and snow, eve.
20	N		Snow showers.
21	W N W	0·5	Ditto.
22	N		Ditto.
23	N N W		Fair.
24	N		Ditto.
25	Var.		Cloudy.
26	S to N W	1·3	Showers.
27	Var.		Rain.
28	W	1·8	Fog, morn; heavy showers.
29	W to N W, a storm		Hail and ditto.
30	N W	0·1	Showers.
31		0·1	Snow ditto, hail, and sleet.
		11·4	Inches rain (and melted snow) this month.

Barometer .... { Highest, 38·16   Wind   E   } Mean, 29·54
               { Lowest, 28·40   N W }

Thermometer .... { Highest, 48°   W   } Mean, 31·83°
               { Lowest, 14   N E and N }

FEBRUARY.

Date.	Wind.	Rain.	Observations.
Feb. 1	N N W		Fair.
2	W to E	0·05	Cloudy morn; showers at eve.
3	N E to S E		Fair morn; cloudy eve.
4	E		Fair.
5	S W	0·33	Light rain, morn; cloudy eve.
6	S W to N W	0·14	Ditto, morn; hail, eve.
7	W N W		Misty.
8	W		Ditto.
9	W S W to W N W	0·20	Fog, morn; showers, eve.
10	S W		Fog, morn; misty eve.

Date.	Wind.	Rain.	Observations.
1814.			
Feb 11	S E	0·18	Misty morn ; showers at eve.
12	S W		Fog, fair, fog.
13	N W	·	Hoar frost and fog ; fair eve.
14	N E		Ditto, ditto.
15			Cloudy (high wind).
16			Fair.
17	E		Ditto.
18	E to N W		Hoar frost ; cloudy.
19	N		Fair.
20	N E		Ditto.
21	Var.		Hoar frost ; fair.
22			Fog, morn ; fair day.
23	S E		Cloudy.
24		0·16	Ditto, sleet.
25		0·16	Ditto.
26	S E to E	0·24	Ditto, heavy showers.
27	E to S		Fair day ; cloudy and fog at night.
28	W S W to W N W		Cloudy.
		1·30	Inch rain this month.

Barometer .... { Highest, 30·15 Wind E } Mean, 30·07
{ Lowest, 29·30 W }

Thermometer .... { Highest, 54° S W } Mean, 38·339
{ Lowest, 22 N W }

MARCH.

Date	Wind.	Rain.	Observations.
Mar. 1	N W		Fair ; strong wind.
2		0·10	Hail showers.
3	N N W		Fair.
4	N W to E		Fair morn, hoar frost ; cloudy at eve.
5	E		Cloudy.
6	E to N		Ditto, high wind, morn ; fair eve.
7	N W		Fair morn ; hail, eve.
8	E		Fair.
9			Ditto.
10	N N E		Cloudy.
11			Ditto, very light snow showers.
12			Fair morn ; cloudy eve.
13	N E		Ditto, ditto.
14			Cloudy.
15			Ditto, and fair.
16	E		High wind, cloudy morn ; fair eve.
17			Ditto, cloudy,
18			Ditto, fair morn ; cloudy eve.
19			Ditto, cloudy morn ; fair eve.
20	S E		Cloudy, fair, cloudy.
21	Var.	0 33	Showers, morn ; cloudy and fair eve.
22	S E		Strong wind ; fair.
23	W		Fair.
24	S W	0·28	Foggy, with showers.
25		0·14	Showers.
26	S W to N W	0·10	Fog, morn ; afternoon, showers.
27	N W to S W		Cloudy day.
28	E to S E	0·24	Ditto, showers.

Date.	Wind.	Rain.	Observations.
1814. Mar 29	S E to S		Ditto, morn; eve, fair.
30	S to S E		Fog, morn; cloudy and fair.
31	S S E	0·12	Strong wind; thick weather.
		1·31	Inch rain this month.

Barometer .... { Highest, 30·37   Wind   E   } Mean, 29·751
                  { Lowest, 28·93   N W

Thermometer .... { Highest, 58   S E   } Mean, 40·371
                 { Lowest, 24   E and N N E

APRIL.

Date.	Wind.	Rain.	Observations.
Apr. 1	S to S E	0·31	Showers, high wind.
2	S S W to S W	0·12	Hail showers and ditto,
3	W S W to N W		Fair.
4	N		Ditto.
5	Var.		Fog, morn; fair day.
6	S S W		Misty.
7	E S E		Ditto, morn; fair afternoon,
8	E		Fair.
9			Ditto.
10			Ditto, high wind.
11			Ditto, ditto.
12	Var.		Fog, morn; fair day.
13			Cloudy and fair.
14	S E		Ditto, ditto.
15	S E to S S W	0·45	Heavy rain, morn; showery eve.
16	S S W	1·35	Thick weather, heavy rain.
17	S W to S·E	⎫	Fair morn; cloudy, with *light* rain, eve.
18	S E	0·06	Cloudy and fair.
19	S S E	⎭	Cloudy, fair, shower.
20	S to N W	0·25	Ditto, ditto, showers.
21	N W	⎱ 0·24	Showery day.
22	N W to E	⎰	Hail showers, morn: fair eve.
23	S W to N W	⎱ 0.61	Showery day.
24	N W	⎰	Hail showers, high wind; fair eve.
25	N W		High wind, fair.
26	N		Fair.
27	S to S S E	⎱ 1·12	Cloudy morn; light rain at night.
28	S to S E	⎰	Wet day.
29	S E	0·50	Ditto.
30			Misty weather.
		5·01	Inches rain this month.

Barometer .... { Highest, 30·27   Wind   E   } Mean, 29·799
                  { Lowest, 29·30   S W

Thermometer .... { Highest, 71   Var.   } Mean, 51·15
                 { Lowest, 33   E

MAY.

Date.	Wind.	Rain.	Observations.
1814.			
May 1	S		Misty morn ; fair eve.
2	E		Fair.
3			Ditto.
4	E N E	0·05	Shower, morn ; cloudy day, high wind.
5		0·25	Gale of wind, morn ; high wind, showery eve.
6	S to E		Misty morn ; fair afternoon.
7	S S W		Fair morn ; misty afternoon.
8	Var.		Cloudy and fair.
9	E N E		Fair morn ; high wind, and cloudy eve.
10			High wind, but fair.
11			Ditto, ditto.
12			Ditto, ditto.
13	N W to N		Cloudy and fair.
14	Var.		Fair.
15	S		Cloudy and fair.
16	S S E		Cloudy.
17	S		Fair.
18	E		Ditto.
19	N E		Ditto.
20			Cloudy and fair.
21			Ditto, ditto, high wind.
22			Ditto, ditto, ditto.
23	N to N W	0·01	Fair morn ; cloudy and light rain, eve.
24	N W	0·03	Fog, morn ; cloudy and showery eve.
25	Var.		Fair.
26	S		Ditto.
27	S to E		Ditto.
28	E to N W		Ditto.
29	N W to S W		Ditto.
30	S W		Ditto, morn ; cloudy eve.
31	E to S		Cloudy morn ; fair eve.
		0·34	Inch rain this month.

Barometer .... { Highest, 30·34	Wind E N E }	Mean, 29·916
{ Lowest, 29·13	E N E }	
Thermometer .... { Highest, 72	Var. }	Mean, 51·306
{ Lowest, 38	N W }	

JUNE.

Date	Wind.	Rain.	Observations.
June 1	E to W	0·61	Heavy rain, morn ; cloudy eve.
2	S W		Cloudy and fair day ; misty eve.
3	E S E	0·32	High wind and showers, morn ; misty eve.
4	S to E N E	0·06	Cloudy, light rain.
5	S	0·01	Ditto, ditto.
6	E		Ditto.
7			Ditto.
8	N E to N W		Fair morn ; cloudy eve.
9	E		Cloudy and fair.
10			Fair morn ; cloudy eve.
11		0·26	Ditto ; showery eve.
12	W S W to E S E		Ditto ; cloudy eve.

Date.	Wind.	Rain.	Observations.
1814.			
Jun.13	E S E to S	0·72	Showery day.
14	S E	0·37	Fog, morn ; fair, mid-day ; *thunder storm*, eve.
15	S W to N W	0·06	Dit o, ditto ; cloudy and fair day.
16	W N W	0·15	High wind ; showery morn ; fair eve.
17			Fair.
18	N W	0·04	Light rain, morn ; cloudy eve.
19	N W	} 0·06 {	High wind, shower ; cloudy and fair eve.
20			Ditto, ditto, ditto.
21			Ditto, fair.
22	N N W		Fair.
23	S W		Ditto.
24	Var.		Ditto.
25	N N W		Ditto.
26			Ditto.
27	N N W		Fair day ; cloudy at night.
28	N W		Cloudy.
29	S W to N W		Fair day ; cloudy at night.
30	Var.	·	Fair.
		2·66	Inches rain this month.

Barometer .... { Highest, 30·36 Lowest, 29·58   Wind   Var. E S E Misty } Mean, 29·959

Thermometer .... { Highest, 74 Lowest, 42   S W N E to N W } Mean, 57·55

ARTICLE IX.

On the Poisonous Qualities of Meadow Saffron. By Mr. John Want, Surgeon to the Northern Dispensary, and Editor of the Medical and Physical Journal.

I have just seen in a morning paper a statement of some year-lings having been poisoned by the eating of meadow saffron. That this assertion is a mere fabrication will be obvious, when it is con-sidered that at this season of the year no part of the plant is above ground, and consequently it is not within their reach. I am in-debted for this fact to Sir Joseph Banks, and Mr. Andrew Knight, the President of the Horticultural Society, and to the latter of these gentlemen for some particulars on this subject too important to be withheld from the public. Cattle are affected by this plant only at the spring of the year, when the seed-vessel is fully matured. It appears that the seed if swallowed adheres to the coat of the stomach, producing at the several points of adhesion spots of in-flammation, which occasion the death of the beast. The account of the coagulated state of the blood found in the stomach is also incorrect, as no such effect is witnessed where the poison has ac-tually been taken. It is a curious fact, that cattle are affected by

the recent seed only. In a dried state, when made into hay, it loses its deleterious property. Whether if taken in any large quantity it would be attended with danger I have not been able to ascertain. It seems very probable that the difference in effect between the recent and dried seed may be owing to the latter having lost its property of adhering to the membrane of the stomach, by which it is enabled to mix with, and is diluted by, the food. I have thought it incumbent on me to furnish you with this information, as the account is generally supposed to have been framed for the purpose of bringing into disrepute the meadow saffron, which I have discovered to be an important auxiliary in the treatment of gout; being in fact the basis of the *eau medicinale.* That this drug possesses poisonous powers to a high degree is unquestionable: one of the names given to it by the French is *tue chien,* from its killing dogs; and the Greek writer Dioscorides describes it as being the Ephemerum, supposed to be so called from its property of producing death in one day. It is to *those only* who find they can take the *eau medicinale* with impunity that I recommend this medicine to be taken without professional advice.

As many of your readers are unacquainted with the mode of preparing this remedy, I subjoin the formula for their information :— Take two ounces of the root of *colchicum autumnale,* cut it into very thin slices, and put it into a bottle with four ounces of proof spirits of wine; let them stand together till the virtues of the root are extracted, when the liquor is to be strained for use. This is the exact composition of the *eau medicinale,* the truth of which I have proved in a vast number of cases. It may be procured at a chemist's at a very moderate price; the intrinsic value of the bottle sold for eight and eleven shillings not being *three pence.*

It is proper the public should be informed that I am prepared to prove, that the greater part of the *eau medicinale* sold in this country has been manufactured here, and not imported, as alleged by the venders.

Article X.

Journal kept by Henry Beaufoy, Esq. during an Aerial Voyage with Mr. James Sadler, sen. from Hackney, Middlesex, to East Thorpe, Essex, five Miles from Colchester, August 29, 1811.

As the balloon ascended I was totally unconscious of the motion: it appeared as if the balloon was the only point stationary, and that the earth and the people were suddenly sinking away. The rapidity with which it ascended was such that it prevented every sensation of giddiness, the whole country appearing in the course of a few seconds as one prodigious map. The almost instantaneous transition from the shouts of the spectators, and from the absolute

tumult in which we had been engaged, to the deathlike stillness that
reigned in the upper regions, only broken at intervals by the report
of a cannon at Walthamstow, filled the mind with indescribable
sensations. It appeared difficult to persuade the mind that it was
a reality; and the mixed sensations of delight and astonishment
completely deprived me of the power of expressing my wonder at
the scene beneath the eye. It seemed a dream, and hardly possible
to be a reality.

A few moments, however, were all that I allowed myself to feast
on the delightful scene; for the confusion that had taken place
around the car had compelled those that had taken charge of the
instruments to use very great exertions to convey them to us in the
car. They were accordingly lying in a distressing state of confusion
at the bottom of the car; though, on examination, fortunately with-
out having suffered the smallest injury. As soon therefore as the
usual ceremony of waving the farewell flag could be dispensed with,
I threw off my hat, and proceeded to arrange and suspend the in-
struments. To effect this it was necessary to have both hands at
liberty. I was desirous of disposing of the flag I held in my hand,
and accordingly thrust the staff through the back of the car ; but as
I was obliged to stand upon the seat to fix the barometer sufficiently
high, the flag fell from its situation, and was afterwards picked up
at about a quarter of a mile from the place of ascent, though
neither of us missed it until some considerable time after the acci-
dent. The instruments being fixed in their respective situations,
the next care was to regulate the gauge of the barometer; all
which several occupations consumed the first ten minutes of the
voyage.

After having made the first set of observations, I had an oppor-
tunity of viewing at leisure the prospect from the balloon. The
first and most striking object was the Thames, which was seen
meandering in endless gigantic sinuosities through the long line of
country down as far as the Nore. The ships, and even boats, were
distinguishable on its mirror-like surface with astonishing minute-
ness ; and I have no doubt that, had the ascent been made with re-
ference to that particular object, the number of shipping afloat in
the river and wet docks might have been most accurately counted.

The sun shone full upon the river, and presented at once the
grandest and most delightful sight imaginable. It would be fruit-
less to attempt the description of the scene, though in candour it
must be acknowledged that it agreed precisely with the idea that I
had preconceived, and differed in no respect whatever from the
view from the summit of a lofty situation ; except that it was infi-
nitely more extended in its range ; the eye embraced a larger field
within its scope ; and then that listless sensation of delight which is
derived from the nature of the voyage itself. In short, as has been
already stated, the gratification arising from the situation is altoge-
ther indescribable, but to such as have experienced it themselves.
Though moving with such wonderful velocity, the travellers are

themselves totally unconscious of any motion whatever. They feel themselves floating in a most delightful aeriform fluid, and seeming to convey a most exquisite idea of unlimited elasticity. The extreme elasticity, indeed, was found on this occasion to be materially against the accuracy required in all barometrical observations. The slighest motion on the part of either of us causing a vibration of the quicksilver, in the tube, of an inch, a half, and two inches, which required to be steadied with the hand to bring it to any thing like a stationary point. Finding this to be the case, I noticed each time the two extreme divisions of vibration, and took the mean as the sum to be placed in the barometer column. In no one instance was the barometer stationary; for even when we were both of us perfectly still, the barometer ebbed and flowed with great rapidity, though not to such an extent as in the case already mentioned.

In looking over the country, it gave the idea of an immense map, executed with uncommon neatness; the fields presenting a much livelier and brighter green than the trees. The colours of objects were not in the least changed or affected in any instance that came under observation. In passing over Epping Forest, I was particularly struck with its appearance; it seeemed to consist of a vast number of clumps of something of a very dark green, certainly conveying an accurate idea of what it really was—a forest; but so much fore-shortened as to preclude any idea of comparative elevation. It occurred to me at the very moment of my noticing it, that although Captain Snowdon had been much joked for having described Epping Forrest as looking like a gooseberry bush, the error really existed by no means in the point of fact, but in the unfortunate selection of words in which he had chosen to express himself; for had he said that Epping Forest looked exactly like a large plantation of gooseberry trees of a gigantic size and width, he would have conveyed a very accurate idea of the fact. I particularly noticed that the forest presented to the eye a tract of dark green detached patches; where the turf (as I supposed) was visible, there seemed to be an edging of varied extent of courses of a green of a much brighter colour. All objects, of whatever kind, ceased to give any idea of comparative height, unless when seen at a considerable angle, before the balloon became in a vertical situation. I observed that white objects, as Chigwell and Ongar Church, Wanstead House, and the Town Hall at Chelmsford, conveyed a much better idea of our elevation above the surface of the earth than any other objects I observed. The small rill of water that runs through the main street of Chelmsford sparkled with peculiar brilliancy; much more so indeed than either the Thames or any other water that caught the eye in the course of the voyage. Such of the roads as took the attention seemed all of one uniform colour, and that an orange-yellow; and, at the elevation at which the balloon was at the time, conveyed the idea of fine gravel walks. In one instance, in which a flock of sheep were passing in a direction from London, the dust they left behind them was very distinguish-

able, and this at an elevation of nearly 3000 feet. All sounds seemed to be transmitted with distinctness to us aloft, at a distance in which we could not make ourselves heard by those under us. This was to be expected, as there could be no objects near enough to the balloon to assist in reflecting the sound; whereas to those beneath us this objection did not apply; the hills and hollows all tending to influence the propagation of sound on the earth. It did not appear that any change in the state of the atmosphere affected the propagation of sound. This was contrary to my expectations. For some years since, when Colonel Beaufoy was out on a shooting party on one of the Swiss mountains, in company with the late Sir Harry Mildmay, they were enveloped in a very dense cloud; by accident Sir Harry's fowling-piece went off, and the report was instantly followed by a complete roll, like that of thunder. The experiment was repeated again and again with similar results. Colonel Beaufoy waited there some time, till the cloud had cleared away, and the ordinary clearness of the atmosphere was restored. He again tried the effects of the discharge of his piece; but now, *no roll* followed.

		Time.		Barometer.			Therm.	Kater's Hygrom.		Electro-meter.	Compass.
		Miu.	Hours.	Inches and Tenths.		Feet,	Degrees.			Divergency	Course of Balloon.
Obs. 1		20	b 3	30	1	—	65	4	30	None	S W wind
	2	10	b 3	26	7	3411	68	4	32	None	N E
	3	5	b 3	26	7	3411	61	4	38	None	N E
	4		3	26	5	3529	59	4	40	None	N B
	5	5	a 3	26	3	3741	56	4	56	None	N E
	6	10	a 3	27	0	3052	66	4	55	None	N E
	7	15	a 3	28	2	1812	60	4	59	None	N E
	8	20	a 3	27	5	2519	61	4	62	None	E
	9	25	a 3	26	4	8771	59	4	65	None	Stationary
	10	30	a 3	25	6	4494	56	4	67	None	Stationary
	11	25	b 4	24	3	5861	54	4	75	2 Tenths	E S E
	12	22	b 4	24	4	5727	54	4	85	1 Tenth	N E
	13	15	b 4	26	0	4032	56	4	90	None	N
	14	10	b 4	26	2	3820	56	4	90	None	E
	15	7	b 4	26	0	4032	57	4	87	None	E
	16	5	b 4	27	0	2986	56	4	86	None	E
	17		4	27	1	2887	57	4	84	1 Tenth	N E
	18	5	a 4	28	5	1501	59	4	83	2 Tenths	N E
	19	10	a 4	Descended in the parish of East Thorpe, near Colchester.							

Remarks made at the different Periods of the above Observations.

Observation 1 made at Hackney Wick, at the moment the balloon was seen rising over the trees, and as the data from which the experiments were to be made during the voyage. The first 10 minutes were occupied in fixing the instruments, and regulating the gauge of the barometer. Mr. Sadler directed me to attend solely to the observations, and that he would himself look to the management of the balloon. Assisted in putting to rights and coiling away rope, grapnel, &c. &c. which were lying in a confused

heap in the bottom of the car. Stuck the flag-staff through the back of the car, and threw off hat.

Obs. 2.—Threw out two bags of ballast, and soon after a third. A most enchanting view. Mr. Sadler pointed out some high chalk cliffs, which he said were the Nore.

Obs. 3.—The balloon had a rotatory motion, which tended to confuse any very distinct idea of situation. This motion most probably caused by some accidental twirl in the confusion in which the balloon was launched.

Obs. 4.—Sent off one of the pigeons, marked No. 7, which the instant it was at liberty flew boldly from the car in a circle, and then towards the earth at a very considerable angle. View clear and distinct.

Obs. 5.—Mr. Sadler uncorked a bottle of Champaigne, and we drank the health of the Prince Regent, and afterwards that of Sir Daniel Williams, followed by All Friends at Hackney. Did not perceive any alteration in the senses of taste or smell, either in the wine, or in some sandwiches. On removing the cork, the fixed air escaped from the bottle in the form of a rather denser kind of smoke, and the wine sparkled with more vivacity than I had remarked on uncorking Champaigne on other occasions. It appeared that the gas escaped with greater facility under the diminished pressure of the atmosphere at this elevation.

Obs. 6.—Observed that the least motion caused by us occasioned an amazing vibration of the quicksilver in the barometrical tube, sometimes considerably more than an inch. The compass-needle not at all altered from its horizontal position.

Obs. 7.—Mr. Sadler tried the effect of the valve, to ascertain whether it was in good order. The gas made its escape through the valve with a noise precisely similar to that of weak steam rushing through the valve of a steam boiler.

Obs. 8.—The balloon was now in the midst of a heavy shower of rain, which was presently changed into a violent hail-storm. The sound produced by the battering of the hail and rain against the upper surface of the balloon, contrasted with the general stillness that otherwise reigned around the balloon, was very striking. Threw out a board which had been taken up to answer the purpose of a table, but not used, because the weight of the load caused the angle formed by the ropes, by which the car was attached to the netting, to become more acute, and we were apprehensive that the edges of the board would cut the ropes. Threw out the wicker basket. The effect of the rain and hail on the balloon was exhibited in a copious discharge of fluid through the neck of the balloon, arising probably from a condensation of the warm hydrogen gas, by the constant succession of cold fluid pouring in torrents on the upper surface of the balloon. This fluid appeared to have dissolved a portion of the varnish; for wherever it fell on the clothes or hat it left a permanent stain of a whitish-looking gummy appearance. At this time we experienced a very strong current of air or wind,

2

not only cold and chilly to the feelings, but apparently blowing from no one particular point of the compass, as it rushed sometimes from one, at another moment from a directly opposite direction. This current of air caused the balloon to acquire a rotatory vertical motion, which made the compass traverse as nearly as I could guess, for I did not note it by the watch, once in about 20 or 30 seconds. The confusion round the car at the launching was here productive of inconvenience; for the car did not hang perfectly parallel. I was at the lowest end, and therefore found this vertical motion exceedingly inconvenient. The car was lowest on my right hand; so that it was not only lowest towards that end, but was lop-sided on my right. The motion of the balloon was from my left towards my right hand. The wind made no noise, and would not have been perceptible but for the freshness of the air on the face, and the singular motion of the balloon.

Obs. 9.—Mr. Sadler now announced to me that the balloon was passing through the clouds; and almost immediately after the clouds were seen beneath, presenting the appearance of fleecy masses. On throwing some small pieces of silver paper over the side of the car, the rapidity with which they appeared to be precipitated downwards convinced us that the balloon was rapidly ascending. The rain still continued, and the air damp and chilly to the feelings. We seemed to be stationary, as far as progress over the country went, but still ascending with rapidity.

Obs. 10.—At this time placed a pigeon, No. 3, on the edge of the car: the poor animal seemed excessively alarmed, standing on the edge of the car and looking round. The earth was concealed from the view by the clouds beneath. After some little time I precipitated the pigeon gently from its perch, when it fell like a stone, until lost in the haze, which was almost in an instant. As long as it remained in sight, it did not make any attempt to assist itself with its wings. The rain still came down heavily, and the fluid continued to pour down as before through the neck of the balloon.

Obs. 11.—Mr. Sadler inquired of me the heat by the thermometer, and on his receiving the answer, directed one of the bottles to be emptied of its water, for the purpose of collecting air: Mr. Sadler observing at the same time that he thought we were now at as great an elevation as we should be able to accomplish in the course of the voyage. At this elevation I could not divest myself of the idea that I heard sounds as of persons cheering from the earth, though it was not possible that it could arise from any such cause, as the balloon was still above the clouds, and we could not distinguish any thing but the dense white clouds, which now appeared precisely like a thick October fog. The air felt damp and chilly, and the rain still continued, though less violently than before. The breath was particularly visible; and from the circumstance of my having been without a hat during the whole of the excursion, it is most probable that the sounds I fancied I heard was merely a ringing in the ears, the effect of the damp. Tried the ex-

periment repeatedly of looking towards the earth, and shouting as loudly as possible to ascertain whether the sound would be returned by echo or reflection from below; but no such effect followed. Got into a clear atmosphere, the white clouds remaining as before beneath; but on looking upwards, there was a mixture of blue and white clouds, though with a great preponderance of blue, just as is usual in a moderately clear day below.

Obs. 12.—The blue sky seemed to be of a dark and clearer blue than I had generally seen. Mr. Sadler now proposed descending into a clearer atmosphere, for the sake of getting a view of the earth, it being still concealed from the view by the dense white clouds below. This was in consequence of our noticing that 22 minutes before 4 the wind had reverted to the old point, and Mr. Sadler's experience led him to conclude that the balloon could not be now far distant from the sea; judging from the rate at which we had traversed over the country, as long as objects were distinguishable. Turned off a pigeon, No. 4, and it would not leave the car, but continued to look about as if frightened, and then turned its head inward, without attempting to escape. When pushed off the side of the car, it fluttered, and used the most violent exertions to regain the car; but as notwithstanding all its exertions it continued to sink rapidly below the car, it at length extended its wings, keeping them apparently immoveable, and darted towards the earth, at an angle considerably inclined, with the rapidity of a hawk making his swoop. It was very remarkable that almost at the same moment a common house fly, apparently much benumbed, and scarcely competent to common exertion, crawled from beneath my seat, and without any difficulty flew with facility upwards, and settled on the lower part of the net of the balloon, a good deal above our heads. It appears curious that so small an insect, and that too partly incapacitated, should be able to fly up to the balloon with the same rapidity as usual, when a far more powerful animal should have sunk from the car almost like a piece of wood thrown overboard. Mr. Sadler now pulled the string of the valve; the gas rushed out with somewhat less noise and violence than before, but the balloon was evidently rapidly sinking: it was a sinking perfectly sensible to the feelings, even had we not been informed by constant reference to the barometer. In ascending, there is a sensation of lifting, or more properly of pressure on the soles of the feet and the under side of the thighs; whereas, in sinking, this sensation disappears. On opening the valve there was a copious discharge of water through the balloon, as before; but it did not appear of so glutinous a nature as that before spoken of : it was probably merely the rain which had lodged on the upper side of the valve.

Obs. 13.—At this time I felt a trifling pressure in the ear, and some little deafness; but this most probably was the effect of the damp atmosphere, and being without a hat; which is by the bye a great inconvenience in such situations, on account of the ropes.

As soon as the balloon descended into a region from which

the earth was perceptible, Mr. Sadler's conjectures proved just; as we saw, apparently at no great distance from us, the wide expanse of the Northern Ocean. The sensation of deafness did not go off for more than a quarter of an hour afterwards, even notwithstanding the balloon had greatly decreased in point of elevation. Until this trifling deafness, there did not appear to be the smallest difference between the intensity of sound at the greatest elevation, and at the surface of the earth. We conversed in our usual tone of voice, and any casual operation, such as drawing the cork of the Champaigne, &c. was heard just as usual. If any thing, the universal stillness invited rather a lower tone of voice than ordinary.

Obs. 14.—Released the pigeon, No. 1, and placed it on the edge of the car, which like the former did not attempt to escape till pushed off from the car.

Obs. 15.—Sent off the pigeon, No. 6 : saw a flock of sheep very distinctly in the turnpike-road, going in a direction from London.

Obs. 16.—Sent off the pigeon, No. 5; Mr. Sadler now announced that it would be necessary to look out for some convenient spot at which to attempt a landing; saw people below at plough; called out to them; but they did not seem to be within hearing, as they did not appear to be aware of the balloon.

Obs. 17.—Mr. Sadler now cautioned me that the instruments must be removed, and directed that they should be taken into my lap. He told me likewise to be prepared, on his giving the word, to heave overboard every thing that would admit of it, with a view of breaking the force of the descent. Mr. Sadler and myself were also to place our feet against the corners of the opposite seat, and then raise ourselves as much as possible with our hands by clinging to the ropes, taking care to raise our hands as high as possible above our heads.

Obs. 18.—Turned off the pigeon, No. 2: this last flew away immediately, but afterwards returned to the balloon, and flew round it several times, but without attempting to settle on the car. The live stock being thus reduced down to one, the bag that contained it was tied to one of the cords of the car, and I then hastened to cut away the ligatures by which the different instruments were secured. In the mean time Mr. Sadler was lightening the balloon of part of a bottle of Champaigne, and emptied out the remaining bottle of water.

The balloon was approaching the ground fast, when Mr. Sadler gave the order to lighten, while he held the valve with both his hands to keep it open. I threw overboard the whole of the remaining ballast, and some two or three other useless articles. Mr. Sadler, when he gave the word for lightening the balloon, at the same instant let go the grapnel. The grapnel continued to drag for a few hundred yards, and I had just time enough to place myself as directed to do, with the instruments secured in the best way the hurry of the moment would admit, when the car bounded from the ground, and after passing over a hedge, and dragging a few feet

more, it lay along on its side. We continued firm in our situations, without attempting to stir, until some persons, who were working close by, in a field over which the balloon had passed in its descent, came to our assistance. The balloon was soon secured, and we were released from the possibility of any farther bumping. The descent was considered by Mr. Sadler as being particularly favourable; though, to speak candidly, I formed a very decided opinion as to the uneasy situation of a descent, which Mr. Sadler, after his long experience, would deem dangerous; for the rapidity with which the car descended through the last 50 or 100 feet on this occasion, and the extraordinary sensation occasioned by the first bound, which is not unlike the dislocating shock of a galvanic battery, very much exceeded my pre-conceived idea as to the nature of a descent. The balloon grounded in a fine grass meadow, in the parish of East Thorpe, near Colchester, and was secured by the assistance of the proprietor of the farm, Mr. Thomas Ely, who was the first person that arrived to lend his friendly aid, and to whose house we, together with our apparatus, proceeded.

On questioning some of the country people who lent a hand in securing the balloon, they told us that they had heard us calling and cheering them as we passed over their heads; and that they had very distinctly seen the water that was emptied out of the bottle, which appears by the journal to have been about five minutes after four. They described it as appearing like a stream of smoke or vapour issuing from the car.

Almost as soon as the balloon touched the ground, a man brought the bottle of Champaigne unhurt, which had been thrown out by Mr. Sadler at an elevation of full 1000 or 1500 feet. The man said he picked it up in a ploughed field. The bottle was about two thirds full, and loosely corked. One of the most remarkable circumstances that I observed was, that the balloon, whether in ascending or descending, provided the change in elevation was effected with rapidity, invariably formed an umbrella over our heads. The lower part, instead of hanging down, as might have been supposed on a first view of the matter, was raised upwards, and formed a concave circle over our heads; the convex side of the arch corresponding with that of the crown of the balloon. This, on reflection, seems to have been caused in both cases by the pressure of the atmosphere. In the descent, the weight below the balloon tended to compress the air against the lower side of the bag, and thus the parachute was formed by the compression of the air, because it could not escape with sufficient ease by flowing over the edges. In the ascent, on the contrary, it is probable that as the upper side of the balloon displaced a much larger portion of air than the lower extremity, in proportion as the balloon when in the air assumes nine times in ten a pear shape, and not a sphere, unless at very considerable elevations, the air which has been so displaced by the upper part of the balloon in ascending, rushes from all directions to re-occupy the space left in the wake of the bag, and therefore it

seems that the parachute thus formed in ascending is merely the effect of the eddy caused by the rapid displacement of the air.

I paid particular attention to this, because it struck me as something curious, which I had not heard mentioned by former voyagers; and I found that, in cases wherein the balloon was nearly stationary in point of vertical change of position, the lower side of the balloon hung down just as would be the case under usual circumstances.

In this voyage we experienced the inconvenience which so often occurs in aerostatic trips in insular situations; the wind being generally in such a situation, with regard to the position of London, as to carry the balloon towards the sea, and not inland. The balloon too that was used on this occasion was only 35 feet in diameter, and had been repeatedly used, and appeared to have not only suffered in the texture, but also to have gained much additional weight, from repeated varnishings.

That it was not at all calculated for the purposes of experiment seemed sufficiently proved by the exceedingly unpleasant smell of hydrogen gas which accompanied us throughout the voyage, and which it was concluded arose from its escape through the little cracks and orifices in the silk and varnish. There is no doubt that any voyage undertaken for the purpose of making experiments should be in a balloon of much greater power than that used on the 29th of August. The utmost elevation attained on this day was very little more than a mile, which is a difference of altitude not capable of exhibiting any variation from general laws sufficient to make it worth while to incur the expense of a journey. That experiments should be made correctly, if they be made at all, no one will be prepared to deny; and therefore it should be considered as a point settled, that not less than two persons should ascend together. The management of the balloon is quite sufficient to engage the attention of one person; and if any thing would tend to shake one's confidence in the extraordinary reports of some aerial travellers, it would be the very fact of their having been alone, and therefore, it is inferred, not by any means so much at their ease, or their undivided attention so much at command, as would have been requisite to read off, for example, the barometrical heights to the nicety they have pretended. It is unnecessary to point out the particular points in which we found that our observations differed or confirmed the reports of others, as most of the excursions undertaken either for amusement or information are pretty generally known. It does not appear, however, that the vertical rotation experienced in the course of this voyage, when the balloon encountered the storm and current of air, has been mentioned by any former travellers, with the exception of Count Zambeccari, who made an ascent with Admiral Sir Edward Vernon, at London, 23d March, 1785.

Article XI.

Table of the Population of Sweden in 1810. From the Memoirs of the Swedish Academy of Sciences for 1813, p. 156.

	Men.	Women.	Total.
Stockholm	29619	35855	65474
County of Stockholm........	45758	50930	96688
Upsala	38040	42537	80577
Nyköping	47177	51486	98663
Linköping	77417	85903	163320
Jönköping	54940	61191	116131
Vexio..........	42726	47251	89977
Calmar	65781	72640	138421
Gottland	15180	17427	32607
Bleking	33348	35847	69195
Christianstad	58548	61963	120511
Malmo	76017	77807	153824
Halland........	35125	38561	73686
Göteborg	57015	59605	116620
Vennersborg	73471	78892	152363
Skaraborg	64872	70617	135489
Carlstad	64934	69874	134808
Orebro	45404	49832	95236
Vesteros	38106	44095	82201
Falun	55320	63332	118652
Gefle	39168	44426	83594
Herrnösand	28353	32147	60500
Jämtland	15979	17200	33179
Vesterbotten	16249	17484	33733
Norrbotten	15461	16941	32402
	1134008	1243843	2377851

Article XII.

Account of an Explosion of Fire-Damp in Hebburn Colliery.
By Φίλελεεινός.

(To Dr. Thomson.)

SIR,

AGREEABLE to my promise, I now send you the following account of the explosion at Hebburn Colliery, which you may

rely upon as authentic. This dreadful accident happened at half past four o'clock, *p. m.* on Friday, the 12th of August, at the mine, when Mr. Elias Mole, the under-viewer, and ten pitmen, had completed a new air-course in a part of the mine, which is locally termed " shifting the air." By this explosion the whole of the above-mentioned persons were instantly killed.

I am informed that this dismal catastrophe was occasioned by the lighted candles of the pitmen coming into contact with the carbureted hydrogen gas, or inflammable air, which abounds in the old workings or waste of the mine, from the ineffectual method of ventilation which had formerly been adopted in this colliery. I believe that great efforts have recently been made to secure this mine from such accidents, by the adoption of a better method of ventilation, and greater attention to the state of the wastes.

It is the opinion of some persons, that the roof of the mine had fallen down and obstructed the air drift, or current, so as to surround the unfortunate men with inflammable air. Others are of opinion that the sufferers had ventured into a part of the mine where the new current of air was not properly established, and that they should not have ventured so far without steel mills. which would have insured a comparative state of safety; or, what would have rendered them completely secure, the lamp of which you gave so full an account in the former numbers of the *Annals of Philosophy*. One individual, Robert Halliday, who was at a distant part of the mine at the time the explosion took place, reports that the concussion of the air was so great as to drive him and another young person several yards along the gallery of the mine, and that every thing was blown down around them, though they were at least three quarters of a mile from the place where the explosion happened. It is considered extraordinary that the explosion did not communicate to all parts of the mine where the inflammable air was accumulated, and at one " fell swoop " destroy every living creature in the colliery; for at other parts of the mine about twenty people were at work, besides the unfortunates. It is to be regretted that the men who were killed were considered the best in the whole colliery; all in the prime of life, and all married men or widowers. And nine widows and 27 children are left to bewail the loss of their valued fathers, who by their hard labour procured them a subsistence.

Permit me, Sir, to mention another explosion, which happened upon the Wear, in Leefield colliery, distant about ten miles from Sunderland, on Friday, the 9th of this month (September), by which four pitmen were killed. The last mentioned accident also arose from a communication having been formed with an old working, where a considerable quantity of carbureted hydrogen gas was accumulated.

    Sept. 12, 1814.

Article XIII.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32′ 40·3″ North.   Longitude West in Time 6″ $\frac{8\ 8}{1\ 0\ 6}$.

1814.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
Aug. 18	8ʰ 45′	24° 12′	50″	1ʰ 35′	24° 23′	46″	6ʰ 55′	24° 17′	53″
Ditto 19	8 25	24 14	01	1 30	24 23	08	7 00	24 16	02
Ditto 20	8 20	24 15	02	1 30	24 22	30	6 40	24 15	45
Ditto 21	8 45	24 21	40	1 25	24 29	38	6 5	24 15	57
Ditto 22	8 35	24 15	08	2 00	24 24	44	7 00	24 16	23
Ditto 23	8 20	24 13	16	1 35	24 21	12	6 40	24 13	15
Ditto 24	8 25	24 16	16	—	—	—	—	—	—
Ditto 25	8 25	24 14	56	1 35	24 23	25	6 45	24 15	50
Ditto 26	8 50	24 14	59	1 40	24 23	12	6 50	24 16	28
Ditto 27	8 30	24 14	53	1 30	24 23	53	6 55	24 17	33
Ditto 28	8 45	24 13	27	1 35	24 24	59	6 42	24 15	30
Ditto 29	8 30	24 13	59	1 35	24 24	48	6 55	24 15	47
Ditto 30	8 25	24 15	14	1 55	24 24	25	6 55	24 17	55
Ditto 31	8 20	24 13	30	1 50	24 25	32	—	—	—

1814.

Mean of Observations in Aug.	Morning	at	8ʰ 30′	..... Variation	24°	14′	13″	} West.
	Noon	at	1 39	..... Ditto	24	23	48	
	Evening	at	6 57	..... Ditto	24	16	31	
Ditto in July.	Morning	at	8 41	..... Ditto	24	13	29	} West.
	Noon	at	1 42	..... Ditto	24	23	44	
	Evening	at	6 58	..... Ditto	24	17	00	
Ditto in June.	Morning	at	8 44	..... Ditto	24	13	10	} West.
	Noon	at	1 30	..... Ditto	24	22	48	
	Evening	at	6 52	..... Ditto	24	16	29	
Ditto in May.	Morning	at	8 45	..... Ditto	24	13	12	} West.
	Noon	at	1 44	..... Ditto	24	22	13	
	Evening	at	6 38	..... Ditto	24	16	14	
Ditto in April.	Morning	at	8 45	..... Ditto	24	12	53	} West.
	Noon	at	1 48	..... Ditto	24	23	53	
	Evening	at	6 29	..... Ditto	24	15	30	
Ditto in March.	Morning	at	8 52	..... Ditto	24	14	29	} West.
	Noon	at	1 52	..... Ditto	24	23	08	
	Evening	at	6 11	..... Ditto	24	15	33	
Ditto in Feb.	Morning	at	8 47	..... Ditto	24	14	50	} West.
	Noon	at	1 52	..... Ditto	24	20	58	
	Evening	at	—	..... Ditto	—	—	—	Not obs.
Ditto in Jan.	Morning	at	8 52	..... Ditto	24	15	05	} West.
	Noon	at	1 53	..... Ditto	24	19	03	
	Evening	at	—	..... Ditto	—	—	—	Not obs.
1813. Ditto in Dec.	Morning	at	8 53	..... Ditto	24	17	39	} West.
	Noon	at	1 51	..... Ditto	24	20	30	
	Evening	at	—	..... Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8 40	..... Ditto	24	17	17	} West.
	Noon	at	1 54	..... Ditto	24	20	24	
	Evening	at	—	..... Ditto	—	—	—	Not obs.

Ditto in Oct.	Morning	at	8	45	..... Ditto	24	15	41	} West.
	Noon	at	1	59	..... Ditto	24	22	53	
	Evening	at	—	—	..... Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	..... Ditto	24	15	46	} West.
	Noon	at	2	02	..... Ditto	24	22	32	
	Evening	at	6	03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	..... Ditto	24	15	55	} West.
	Noon	at	2	02	..... Ditto	24	23	32	
	Evening	at	7	05	..... Ditto	24	16	08	
Ditto is July.	Morning	at	8	37	..... Ditto	24	14	32	} West.
	Noon	at	1	50	..... Ditto	24	23	04	
	Evening	at	7	08	..... Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	..... Ditto	24	12	55	} West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	..... Ditto	24	12	02	} West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	40	..... Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	..... Ditto	24	09	18	} West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
Sept. 1	8^h 30'	24° 14'	02"	1^h 45'	24° 25'	52"	6^h 50'	24° 17'	07"
Ditto 2	8 25	24 12	47	1 35	24 27	55	6 55	24 15	26
Ditto 3	8 30	24 14	58	1 45	24 24	21	6 45	24 15	99
Ditto 4	8 25	24 14	38	1 35	24 28	09	6 45	24 18	37
Ditto 5	8 25	24 13	25	1 40	24 25	06	6 40	24 13	31
Ditto 6	8 35	24 18	59	1 45	24 24	53	—	—	—
Ditto 7	8 35	24 15	08	1 50	24 24	18	6 40	24 15	45
Ditto 8	8 35	24 15	41	1 40	24 22	01	—	—	—
Ditto 9	8 40	24 17	06	—	—	—	—	—	—
Ditto 10	8 20	24 15	10	1 30	24 24	51	6 25	24 16	23
Ditto 11	8 30	24 14	35	1 40	24 24	08	6 35	24 16	23
Ditto 12	8 35	24 41	02	1 45	24 36	51	6 25	24 19	51
Ditto 13	8 40	24 19	09	1 50	24 23	03	6 35	24 14	30
Ditto 14	8 35	24 16	03	1 45	24 25	18	6 25	24 15	10
Ditto 15	8 35	24 15	47	1 45	24 23	36	6 25	24 17	32
Ditto 16	8 35	24 13	14	1 40	24 23	20	6 20	24 17	03
Ditto 17	8 30	24 13	30	1 40	24 20	52	6 20	24 18	12

Rain fallen { Between noon of the 1st Aug. } 2·041 inches.
                  { Between noon of the 1st Sept. }

Aug. 2.—In deducing the mean variation, the morning observation is rejected.

Aug. 21.—The morning and noon observations are rejected. This unusual variation was followed by heavy rain during the night.

Sept. 2.—Noon variation great. Wind fresh, E. N. E.

Sept. 4.—Noon variation great. Wind N. E. Weather cloudy.

Sept. 12.—Variation remarkable, and the morning observation greater than the noon, for which no reason can be assigned. The weather was overcast, and the wind N., blowing at the rate of ten miles an hour.

Comparison of the Variation in the Years 1813 *and* 1814.

		1813.			1814.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	25″
	Noon	24	21	12	24	23	53	+	2	41
	Evening	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	22	13	+	1	19
	Evening	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening	24	16	04	24	16	18	+	0	44
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening	24	16	43	24	17	00	+	0	17
Aug.	Morning	24	15	55	24	14	13	−	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening	24	16	08	24	16	31	+	0	23

Article XIV.

Milk Powder.

In the *Annals of Philosophy* for February last, p. 151, I mention that Kirchoff, a Russian chemist, had proposed a method of converting milk into a powder, in order to preserve it unaltered. The following passages, copied from an old book, for the knowledge of which I am indebted to a philosophical friend, show that this discovery is not new, but was known many years ago.—T.

Observatio Joh. Baptistæ Werloschnigg.

Qui hactenus lac suis medicationibus adhibuerint, crudum, excrementis nondum purgatum et in quintum esse deductum, hinc factum ut sæpius ob sui in ventriculo coagulationem enormia eruperint mala ; et quia plura et plura in medicinâ nostri ignorarunt antecessores, quæ nos apprime nunc novimus, plurima et nos ignoramus quæ successoribus erunt perspecta : sic nemo medicorum præteritis seculis lac in pulverem redigerè, sordibus liberare, illudque cuivis temperamento aptum reddere novit.

Quod tamen excellentissimus vir meus amicus singularis Dominus Ludovicus Testi, Med. Professor Venetiis, optime novit. Hic enim certo spagyrico* mysterio *substantiam lactis in pulverem albescentem dulcem* (quod arcanum opportuno tempore in humanæ miseræ solatium se manifestare promittit) *convertit.* Fabrefacit itaque ex lacte vaccino, melius ex muliebri (verum hoc solum principibus et divitibus foret superabile), saccharum et sal lactis, utrumque in quibus acidum et acre peccat, optatissimum, tutissimum, et

* Chemical.

infallibile remedium, siquidem suis spurcitiis depuratum nec acescit, nec coagulatur, nec corrumpitur, ut facile lacti contingit, et in longos annos asservari potest.—Miscellanea Curiosa ; sive Ephe-meridum Medico-Physicarum Germanicarum Academiæ Cæsareæ Leopoldinæ Naturæ Curiosorum. Decadis tertiæ, Annus 9 et 10. Obser. 155, p. 282.

Pretium Dosis, et Vehiculum Sacchari Lactis.

Dum apud manipulatorem pretium indago sequens scripto dedit responsum.

Quanti hoc remedium divendam, dico a prima usque inventione septimum jam labi annum, librasque fere mille a me fabrefactas, nec tamen unquam granum pretio æstimavi. Generose ego peten-tibus usque ad ultimos Europæ terminos, immo in Asiam transmisi. Necessarium enim fuit, talem me habere, non solum ut non im-postor crederer, verum etiam ut arthritici manifeste comprehende-rent, neglecto lucro, gratam mihi primo esse eorum salutem. Cum modo autem de remedii præstantia ab experientiis confirmata satis constet, æquum etiam est, ut divites congruens aliquod munus mihi respondeant. Necessariam ergo subministrabo quantitatem, dummodo singulis mensibus duo aurei Pannonici seu Ungari ab ægroto dono dentur. Hoc certe tenue munus est, quod arthriticus offerret, si actu ego, nullo quamquam propinato remedio, ac sola personâ inviserem.

Venetiis, 1 *Octobris,* 1701.   LUDOVICUS TESTI, D.

Dos a ʒj. ad ʒjss. ʒji. bis, ter, etiam quater in die ex jure, pulte, vel ex infuso theæ, Veronicæ, &c. sumitur.

ARTICLE XV.

ANALYSES OF BOOKS.

I. *Kongl. Vetenskaps Academiens Handlingar, för år* 1813. *Memoirs of the Royal Academy of Sciences at Stockholm, for* 1813.

THE Swedish Academy was founded in 1739, and Linnæus was its first President. It bears a stronger resemblance to the Royal Society of London in its nature and constitution than any other scientific society on the Continent. The Members are all free to write on what they please, and to contribute or not to the Memoirs of the Society. A new President is elected every half year. They publish two thin octavo volumes of Memoirs annually. The so-ciety consists at present of 90 Swedish, and 66 foreign Members. They are said to possess very considerable funds, and have an excel-lent house in Stockholm, where their meetings are held, and their scientific and natural history collections deposited. Natural history

has always formed a leading department in their Memoirs. I shall here present my readers with the contents of their Memoirs for 1813, the last published volumes, and give outlines of such papers as are capable of abridgment, and suitable to the subjects discussed in the *Annals of Philosophy.*

1. On the velocity of the water, which, according to the experiments hitherto made with small wheels, is found proportional to the rectangular shock. By Zach. Nordmark.

2. On the determination of the time, when the true anomaly is given, and the orbit does not differ much from a parabola. By S. A. Cronstrand.

3. An easy method of determining the principal properties of the curve line in which a body is drawn towards a given point by a centripetal force, which is proportional to some function of the distance. By N. J. Bergsten.

4. Experiments in order to improve theoretical chemistry, and to render its systematic arrangement subservient to a more perfect chemical nomenclature. By Jac. Berzelius.

5. Extracts from a table exhibiting the births and deaths in Sweden during the years 1806—1810. By H. Nicander. These tables are too copious to be inserted here; but they deserve the attention of those who are engaged in the interesting department of political arithmetic. The years included in them were very eventful and very injurious to Sweden.

6. Experiments how, by means of metallic wires, or a collection of metallic points, galvanism may be produced of considerable power and utility for medical purposes. By J. P. Westring.

7. *Rosa centicosa,* a new Swedish species of rose, described by Erik Acharius. This new species grows in East Gothland, and had been previously confounded with the *rosa canina.* The description is accompanied by a figure.

8. An attempt to delineate the colours in natual history. By G. J. Billberg. This is similar to the late publication of Mr. Syme, noticed in the last Number of the *Annals;* but not quite so well executed. Billberg delineates 80 colours, which he divides into 10 sets; namely, white, grey, black, blue, green, yellow, orange, red, brown, and violet, or purple. For the sake of his Latin names, I shall here subjoin his list.

I. White, *albus*; chalk-white, *cretaceus*; snow-white, *niveus*; milk-white, *lacteus.* II. Grey, *griseus*; yellow-grey, *lividus*; white-grey, *canus*; blue-grey, *cinereus*; red-grey, *murinus*; brown-grey, *luridus*; green-grey, *encanus.* III. Black, *ater*; brown-black, *piceus.* IV. Blue, *cæruleus*; black-blue, *atrocæruleus*; azure-blue, *azureus*; dark-blue, *obscurè cyaneus s. cæruleus*; sky-blue, *cælestinus*; smoke-blue, *fumatus*; grey-blue, *cæsius.* V. Green, *viridis*; blue-green, *glaucus*; light copper-green, *malachiticus*; copper-green, *æruginosus*; sea-green, *thalassinus*; black-green, *atroviridis*; olive-green, *olivaceus*; pistacio-green, *pistacinus*; dark-green, *prasinus s. obscurè viridis*; yellow-green,

luteo viridis. VI. Yellow, *luteus*; sulphur-yellow, *sulphureus*; whitish-yellow, *alboluteus*; straw-yellow, *stramineus*; greyish-yellow, *flavus*; egg-yellow, *vitellinus*; dark-yellow, *citrinus s. obscurè luteus*; saffron-yellow, *croceus*; ochre-yellow, *ochraceus.* VII. Orange, *aurantiacus*; lion-orange, *helvolus*; isabella-orange, *isabellinus*; minium-orange, *miniatus*; cinnaber-orange, *cinnabarinus.* VIII. Red, *ruber*; blood-red, *sanguineus*; fire-red, *igneus*; scarlet-red, *coccineus*; crimson-red, *chermesinus*; carmin-red, *puniceus*; tile-red, *lateritius*; yellow-red, *fulvus*; light yellow-red, *testaceus s. clarè fulvus*; flesh-red, *carneus*; light flesh-red, *incarnatus s. pallidè carneus*; rose-red, *roseus.* IX. Brown, *brunneus*; red-brown, *rufus*; rust-brown, *ferrugineus*; light rust-brown, *cinnamomeus s. clarè ferrugineus*, chesnut-brown, *castaneus*; grey-brown, *fuscus*; umber-brown, *umbrinus.* X. Violet, *violaceus*; dark purple-violet, *atropurpureus s. obscurè purpureus*; purple-violet, *purpureus*; lily-violet, *lilacinus.* I have omitted the metallic colours, 12 in number.

9. On the method of determining from experiments on hydrotechnical models in a small scale the work which may be performed by large machines. By Z. Nordmark.

10. Extract from the table drawn up to show the number of inhabitants in Sweden in 1810. By H. Nicander. The sum total is 2,377,851. I have given an extract of this table in the present Number of the *Annals.*

11. Experiments in order to improve theoretic chemistry, with some alterations in the chemical nomenclature. By Jac. Berzelius. These valuable experiments have already made their appéarance, partly in the Annales de Chimie, and partly in Gilbert's Annalen. Some of them have found their way from these journals into the Philosophical Magazine. I have given an abstract of the alterations proposed by Berzelius in the chemical nomenclature in a preceding Number of the *Annals of Philosophy.*

12. Experiments to determine the quantity of oxygen in the oxides of cerium. By W. Hisinger. A translation of this paper will be given in the next Number of the *Annals of Philosophy.*

13. Experiments on the proportion in which bismuth unites with sulphur and oxygen. By P. Lagerhjelm. A translation of this paper will also be inserted in the next Number of the *Annals.*

14. Geographical positions in the districts of Calmar and Ostergötland, determined by observations with a chronometer and sextant in 1812. By C. P. Hällström. The latitude and longitude of 17 places are determined. The following table exhibits the general results.

	N. Latitude.			Longitude West from Stockholm.		
Norrköping	58°	36′	3″	1°	53′	4″
Knappekalla	58	9	12	1	34	3
Hallingeberg	57	50	17	1	48	48

	N. Latitude.				Longitude West from Stockholm.		
Wimmerby	57	40	17	. . . . . .	2	13	26
Ishult	57	28	33	. . . . . .	1	46	27
Staby	57	9	46	. . . . . .	2	2	7
Rosendall	57	10	18	. . . . . .	2	31	45
Molilla	57	23	35	. . . . . .	2	17	16
Bläckebo	56	54	3	. . . . . .	2	0	30
Bronshult	56	46	28	. . . . . .	2	15	14
Calmar	56	40	28	. . . . . .	1	42	19
Mönsteros	57	3	44	. . . . . .	1	37	44
Döderhultsvik . . .	57	16	28	. . . . . .	1	37	18
Westervik	57	46	2	. . . . . .	1	25	31
Lislekust	58	0	6	. . . . . .	1	27	58
Färjestaden	58	37	55	. . . . . .	1	26	17
Sandviken	58	40	21	. . . . . .	1	41	6

It may be proper to mention that the longitude of Stockholm is 18° 5′ 15″ east from the observatory at Greenwich.

15. Description of some water-flies (*hydromyzides*) found in Sweden. By Carl. Fredr. Fallen. The following are the insects described in this paper, which the author arranges under new genera in this manner :—

Hydromyza
     livens. Musca livens of Fabricius.

Ochtera
    manicata. Tephritis manicata of Fabricius.

Ephydra
    1. riparia.
    2. aquila.
    3. coarctata.
    4. stagnalis.

Notophila
    1. caudata.
    2. cinerea.

Notiphila
    3. annulata.
    4. griseola.
    5. albifrons.
    6. glabricula.
    7. obscurella.
    8. madizans.
    9. nitidula.
    10. ænea.
    11. guttata.
    12. picta.
    13. punctata.
    14. interstincta.
    15. quadrata.

16. A new bird (*rallus paykullii*) described by Sven Ingemar Ljungh.

17. Observations on the application of a weight (*tamponers*) to stop the blooding of the nose. By C. Travenfelt.

18. Observations on the efficacy of gold as a medicine. By Joh. L. Odhelius.

19. Biographical account of Shering Rosenhanes, Secretary of State.

20. Biographical account of And. Gust. Ekeberg. Translated in the present Number of the *Annals of Philosophy*.

21. Biographical account of Peter Jacob Hjelms.   A translation of this will be inserted in the next Number of the *Annals.*

II. *An Essay on Dew, and several Appearances connected with it.* By William Charles Wells, M. D. F. R. S. London. Taylor and Hessey. 1814.

DR WELLS was induced, in consequence of a rude experiment made in 1784, to think it probable that the formation of dew is attended with the production of cold. Mr. Wilson and Mr. Six entertained the same opinion : and Dr. Wells, in the course of his reading, met with many facts that appeared to confirm it; but he made no experiments on the subject till the autumn of 1811. The facts then observed induced him to begin a series of observations in 1812 ; these were prosecuted in 1813, and gradually led to a knowledge of the facts contained in this publication, which may be considered as a theory of the formation of dew—a theory which appears to me both satisfactory and complete.

The essay is divided into three parts. In the first we have an account of the *phenomena of dew.* It appears only in calm and serene nights. In cloudy nights the quantity is small, and the same observation applies to windy nights; and when the night is both cloudy and windy, no dew whatever is deposited. It usually begins to make its appearance on grass towards the decline of the day, when the sun has ceased to shine on it; but it does not appear in any considerable quantity till after sun-set. It continues to be deposited during the whole of the night till after sun-rise, supposing no change in the weather to have taken place. There is usually a greater quantity deposited between midnight and sun-rise than between sun-set and midnight. Supposing every thing else the same, the deposition of dew is more abundant the more the atmosphere is loaded with moisture.

Whatever has a tendency to diminish the *aspect* of the sky to any body diminishes at the same time the quantity of dew deposited on it.   Thus, a flat board was placed in a horizontal position four feet above a grass plat; 10 grains of wool placed on the under surface of it gained only four grains of weight, while the same quantity placed on the upper surface gained 14 grains. In another trial the wool on the under side gained six grains, while that on the upper side gained 19 grains. In another trial the wool on the under side gained two grains, while that on the upper side gained 11 ; and in another the wool below gained four grains, while that above gained 20. This difference was not owing to the moisture falling from above in a form similar to that of rain, otherwise the wool on the under surface would have acquired no increase of weight.   Besides, a quantity of wool placed at the bottom of a tall stone-ware cylinder, open at top, acquired just as little increase of weight as the wool on the under surface of the board.

When wool is laid upon a large horizontal body, as a board, it acquires more dew than when suspended in the air at a distance

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from other bodies. Dew is not deposited indiscriminately upon all bodies. More is deposited on grass than upon gravel or soil. Polished metals have but little deposited on them, so that they are often found dry when other bodies in their neighbourhood are wet with dew. Between metals themselves there is a difference in this respect : some being more readily wetted with dew than others. Platinum is one of the easiest. Iron, steel, zinc, and lead, are more easily wetted with dew than gold, silver, copper, and tin. Dew is deposited in towns as well as in the country, but not so copiously, owing to a variety of causes which may be easily conceived. Thus, Dr. Wells found dew deposited in London, but in much smaller quantity than in a garden in Surrey, about a mile and a quarter from town.

If we examine the temperature of those substances on which dew is deposited, we shall find them colder than the air in their neighbourhood. Thus, grass wet with dew was found 7°, 8°, 9°, 10°, 11°, and even 12° colder than the air four feet above it. This difference of temperature begins soon after the heat of the day has declined : but this difference only holds in clear and serene weather. In cloudy and windy nights the grass was not colder than the air : sometimes it was even warmer ; but on such nights no dew was deposited. If the sky become cloudy during the night, the temperature of the grass rises. Thus, on such a night, the grass which was at first 12° colder than the air became only 2° colder. During a fog this increase of temperature sometimes takes place, sometimes not ; showing clearly that the change does not depend upon the fog, but the cloudiness of the sky.

The quantity of dew deposited upon bodies is proportional to their coldness, when compared with that of those in their neighbourhood. Thus, wool on the upper surface of a board was 9° colder than wool upon the under surface of the same board, and it condensed more than three times as much dew. Much more dew is deposited upon grass than upon a gravel walk. On one night in which this difference was very striking, the grass was 16½° colder than the gravel walk. The same observation applies to garden mould, which does not condense so much dew, nor become nearly so cold as grass.

On dewy nights the temperature of the earth an inch or an inch and a half under the surface is much warmer than that of the grass. In one case the observed difference was from 12° to 16°.

The heat of metals is not correctly ascertained by placing the naked bulb of a thermometer on them. Unless the bulb be covered by a coating of gilt paper, or something similar, it indicates a much greater degree of cold than the metal really has.

Bright metallic plates on dewy nights are much warmer than the grass on which they are placed, and usually as warm as the air four feet above them : but when dew forms on them they are always colder than the air. But metals do not become so cold as other bodies by exposure : they were never observed more than 3° or 4°

colder than the air; and the most readily dewed metals are the ones which become coldest soonest.

The quantity of dew deposited depends not merely on the difference between the temperature of the air and of those bodies on which the dew condenses; it depends likewise, and in a very considerable degree, upon the quantity of moisture which exists in the atmosphere.

When wool is equally exposed it becomes colder than grass; while silk, cotton, and flax, become colder than wood. Swandown becomes still colder than any of these bodies. Fresh unbroken straw, and shreds of white paper, were found, likewise, to become colder than wool. Of powders, fine river sand became the least cold; glass, and chalk, in powder, became more so; and charcoal, lamp-black, and brown calx of iron, became coldest of all. Glass, brick, cork, and oak-wood were inferior to filamentous substances. Snow likewise became a good deal colder than the air; but in this respect it is inferior to swandown.

Such are the phenomena observed by Dr. Wells to accompany the formation of dew. In the second part he treats of the *theory of the formation of dew.* Aristotle supposed that dew was a species of rain, formed by the moisture of the atmosphere being condensed by the cold of the night; an opinion still adopted by Mr. Leslie. The discovery by Muschenbroek, that metals will be free from dew while other bodies attract it copiously, led to the conclusion that the formation of dew is an electrical phenomenon; that it is deposited on non-conductors, but not on conductors. Mr. Wilson and Mr. Six conceived that its formation was accompanied by the evolution of cold; an opinion at first embraced by Dr. Wells: but subsequent observations led him to doubt its accuracy, and he afterwards ascertained, by direct experiment, that the temperature of bodies sinks before any dew is deposited on them; that the subsequent deposition of dew is the consequence of this coldness; and therefore that the deposition of dew has precisely the same cause as the appearance of moisture on the outside of a glass or metallic vessel, when a liquid considerably colder than the air has been poured into it shortly before.

But why, it will be asked, do bodies become colder than the air with which they are in contact; and why do some bodies acquire a greater degree of cold than others in their vicinity? These questions could not have been answered in a satisfactory manner previous to the discoveries of Mr. Leslie and Count Rumford respecting heat. But in consequence of these discoveries we are able to answer them in a satisfactory manner. All bodies have the property of radiating heat. During the day, the heat lost by radiation is more than supplied by the sun; so that the temperature of bodies during the day is increased instead of being diminished. But the contrary is the case during the night. The heat radiated by the bodies on the surface of the earth penetrates into the sky, and does not again return to them. Hence their temperature must be constantly diminishing

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from radiation; and as air is a very bad conductor, the heat thus lost cannot be supplied by the ambient atmosphere. Therefore the temperature of bodies at the surface of the earth will become and continue colder than the air during the whole night. But this will happen only when the sky is clear, and the atmosphere still. If the sky be covered by clouds, these opaque bodies will radiate back as much heat as they receive from the bodies on the surface of the earth; so that the temperature of these bodies will not sink. If the night be windy, the agitation of the atmosphere will in some measure make up for its bad conducting power. New and warmer portions of air coming continually in contact with the bodies on the surface of the earth will be continually supplying them with heat, and thus prevent their temperature from sinking from radiation.

Thus we see a sufficient reason why the temperature of bodies on the surface of the earth in clear and calm nights becomes considerably lower than that of the surrounding atmosphere. But what is the reason that the temperature of some bodies becomes much lower than that of others in their immediate vicinity? The discoveries of Mr. Leslie enable us to answer that question in a satisfactory manner. It is because some bodies radiate much more heat than others. Metals are the bodies which radiate heat worst, of course they will be less cooled by radiation than other bodies, and less dew will of course condense on them, as is the matter of fact. Gold, silver, copper, and tin, are the metals that radiate heat worst, and they are the metals upon which dew is least apt to form. Filamentous substances, paper, grass, radiate heat copiously: hence they become speedily colder than the air, and dew forms upon them in abundance.

Such is the theory of the formation of dew given by Dr. Wells, and, to me at least, it appears perfectly satisfactory and complete. He employs it to explain a great number of facts, some of which he had mentioned in enumerating the phenomena of dew, and others are noticed for the first time here. I would with much pleasure run over this enumeration of facts, and the luminous explanation of them given by Dr. Wells, which in most cases is perfectly satisfactory; but want of room obliges me to refer the reader to the Essay itself: and I do this with the less reluctance, because most of the phenomena explain themselves as it were spontaneously by the mere application of the theory to them.

The third part of this Essay treats of *several appearances connected with dew.* By this, Dr. Wells means several appearances produced by the same cause as dew, but generally ascribed to other causes. These appearances are the following:—

1. During winter, Dr. Wells observed the panes of the windows of his bed-chamber moist on the inside; but those which had been covered by an inside shutter during the night were much more so than those that had been uncovered. He found upon examination that the covered panes were 3° colder than the uncovered. The cause was this:—The shutter protected the panes from the heat

radiated by the walls and furniture of the room, which struck against the uncovered panes; hence the former were colder than the latter.

2. When we go out of a house into the open air during the night, we are frequently sensible of a considerable chill. This, according to Dr. Wells, is greatest in a clear night, and is more sensible in the country than in town. He conceives it to be owing to the heat radiated from our bodies. To this cause the noxious effects of the night air are probably owing.

3. Gardeners are in the habit of covering plants with mats in order to prevent the bad effect of the cold. The use of this covering is to prevent the heat from escaping from the plants by radiation. Dr. Wells found grass, over which a thin cambric handkerchief was placed horizontally a few inches above it, several degrees hotter than grass fully exposed to the aspect of the sky. The effect is not so great when the mat touches the body as when it is a few inches distant from it.

4. The covering of snow upon the surface of the earth during winter in northern regions is chiefly useful by preventing the loss of heat from radiation; which would be sufficient in a few hours to destroy vegetables altogether.

5. It is believed in the West Indies, and seems to have been admitted by the ancients, that the exposure of animal substances to moonlight promotes their putrefaction. Moonlight nights are always clear, and usually calm: hence dew will be deposited most copiously during such nights. Covering animal substances with dew, in addition to their own moisture, must in hot climates promote their putrefaction: hence probably the origin of the notion.

6. Ice is formed in Bengal by exposing water in shallow unglazed pans to the air, placed upon dry straw or sugar-canes, and sometimes sunk artificially below the surface of the earth. Ice forms only in clear and serene nights. The formation of ice in these cases has been usually ascribed to the evaporation of water from the outer surface of the pans. But Dr. Wells has shown, both by the clearest arguments and by experiments, that this explanation is not the real cause; but that the formation of ice is owing to the heat radiated from the surface of the water. The straw serves only to prevent heat from being conducted into the water from the earth. Hence the reason why the process requires a clear and serene night, and why wind and clouds prevent it from succeeding.

Such is a short analysis of this very interesting performance. Few books have made their appearance of late years containing a greater number of new and important facts; and few scientific theories have ever been presented to the world either in a more satisfactory or luminous manner. It may be proper to mention that there occurs a word in Dr. Wells's essay which I do not recollect to have ever seen before. I mean the word *conduction*. The English language was destitute of a single word bearing the meaning

which Dr. Wells has affixed to this. I have frequently in writing or speaking on the subject found the inconvenience of such a want, and was obliged to have recourse to the disagreeable mode of expressing my meaning by a circumlocution. Whether the word *conduction* be the most proper for the purpose must be left to the decision of British philosophers in general. For my own part, I see no objections to the term, and should not scruple to employ it when writing upon heat.

Article XVI.

Proceedings of Philosophical Societies.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

PHYSICS AND CHEMISTRY. By M. le Chevalier Cuvier.

It is supposed that the combinations which exist commonly in nature are those to which the great affinity between their constituents give a certain stability, and which circumstances by no means common have the power to decompose; while those which do not possess that property can only be momentary and accidental productions, or obtained by the labours of chemists. The greater number of the former are discovered, the more fugitive ought those to be which remain to be discovered, and the more subject to be destroyed by the least accidental circumstance. This is what has occasioned the accidents of which the history of chemistry offers so many examples, and against which the more precautions ought to be taken the more difficult and delicate the researches are in which the chemist is engaged.

M. Dulong, Professor of Chemistry at Alfort, was on the point last year of becoming a victim to his zeal for the science. But his danger was recompensed by a fine discovery—a compound of azote and oxymuriatic acid, which presents the most singular properties. To obtain it we must present to the *chlorine,* as the British chemists at present term it, azote not in the state of gas, but in any combination. Any ammoniacal salt will answer, provided the acid which it contains be not driven off by the chlorine. M. Dulong passes a current of chlorine through the solution of such a salt, and obtains a kind of brown oil, heavier than salt water, which is quickly dissipated in the open air, and detonates when heated with a report louder than a musket. Copper decomposes it, uniting with the chlorine, and disengaging azotic gas. Hence we see its constituents.

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But what renders it dangerous to examine this substance is, that when it comes in contact with any combustible substance, as with phosphorus, a violent detonation takes place, which breaks all the vessels. This is a new example, and, as appears, the most energetic of all, of those combinations in which the heat that kept the substances in the gaseous state still continues united with them, though they have become liquid or solid; a case which chlorine presents more frequently than any other body. M. Dulong proposed to ascertain the proportions of the two constituents of this substance, and its manner of acting on other bodies, especially on the metals; but the accidents which this young chemist met with at two different times, one of which deprived him of an eye, put an end to his experiments; and for the sake of the science, to which his sagacity may still be of considerable service, the Class has engaged him to turn his attention to other subjects.

This same substance almost deprived chemistry of one of its most illustrious cultivators, Sir Humphry Davy, formerly Secretary of the Royal Society, who, though still young, has already made numerous and brilliant discoveries, and particularly that of the metallization of the alkalies and earths, which opens a new field to so many branches of the natural sciences.

A substance not less remarkable has presented itself to M. Courtois, manufacturer of saltpetre at Paris. MM. Clement and Desormes have shown it to the Class, and M. Gay-Lussac has made upon it a set of instructive experiments. It is obtained from the mother water of kelp by sulphuric acid and distillation. When cooled and condensed, it has the appearance of plumbago. Before it has been purified it melts at 158°; but when purified by solution in an excess of potash and distillation, it requires a much stronger heat to melt it. Its most striking property is being converted by heat into a violet coloured vapour, or gas, perfectly homogeneous and transparent. Neither a red heat, nor oxygen, nor charcoal, act upon it. It combines with the metals and their oxides, and these combinations are soluble in water. With ammonia it produces a fulminating powder. Sulphureted hydrogen discolours it, and converts it into a powerful acid; from which it may be again precipitated by chlorine, sulphuric or nitric acid. In fact its action on other bodies is so similar to that of chlorine, that it may be explained in a similar manner. We may either suppose it a peculiar acid combined with oxygen; or, with Davy, we may conceive it to be a simple substance capable of forming a peculiar acid by uniting with hydrogen. According to the first way of viewing it, we must suppose that the hydrogen unites with the superabundant oxygen, and forms water, which cannot afterwards be separated from the acid. What induced Davy to alter the received theory of oxymuriatic acid was, that hydrogen converts it into ordinary muriatic acid, without its being possible to obtain any water. Yet water ought to have been formed if the hydrogen merely deprived the

chlorine of a quantity of oxygen. Davy applies a similar theory, founded on the same analogies, to the fluoric combinations.

This celebrated chemist, recently named a corresponding member of the Institute, has presented a paper upon this new substance, in which he insists upon its analogy to chlorine, and upon the motives and reasons which induce him to consider both of them as simple substances, capable, like oxygen, of burning and acidifying combustible substances. Thus when the new substance (which has been called *iode*, from the colour of its gas,) combines with potassium, it exhibits a fine blue flame, but no gas is disengaged ; but if potassium be dissolved in iodic acid, hydrogen gas is disengaged. The same thing holds with the other metals. Davy ascribes the formation of this acid by phosphorus to the humidity which always adheres to the iode, and which it decomposes. He was not able to procure oxygen from iode nor its acid, nor to make oxygen act upon the one or the other ; nor had they any action on carbon; nor was iode decomposed by the galvanic battery. But iode, like chlorine, forms triple compounds with the alkaline metals and oxygen, which detonate with charcoal, and may be employed instead of nitre.

The detonating powder obtained from iode by MM. Clement and Desormes by means of ammonia is, according to Davy, a compound of iode and azote; so that it is analogous to the dreadful substance discovered by M. Dulong, which is a compound of azote and chlorine.

Another manufacturer, enlightened by chemical science, M. Tassaert, has made a remark which may be of consequence in the arts. The floors of his soda furnaces are composed of sandstone. On pulling them down he observed a blue matter, which never made its appearance when the furnaces were built of brick, and in which Vauquelin found nearly the same constituents as in ultramarine ; so that our learned associate does not despair that, by following out this process, we shall be able one day to imitate nature in the production of this very precious article. M. Pelletan, jun. has remarked that there appears in many cases during the manufacture of soda, a blue colour more or less intense, which is not destroyed by calcination, and that this colour appears principally when iron comes in contact with soda not yet entirely deprived of its sulphuric acid.

Crude platinum as it comes from Peru is a very compound substance. Besides pure platinum, a noble metal, heavier, and equally unalterable with gold, it contains iron, copper, mercury ; and the successive researches of Wollaston, Tennant, Descotils, Fourcroy, and Vauquelin, have shown ten years ago the presence of four metals, distinct from all others previously known, and called *palladium, rhodium, osmium,* and *iridium.*

M. Vauquelin has this year resumed the study of these substances, and has read a memoir on the most convenient methods of obtaining *palladium* and *rhodium* in a state of purity.

After having precipitated the greatest part of the platinum from the nitro-muriatic acid solution by means of ammonia, he puts into the residue plates of iron, which throw down the other metals. Employing in succession cold nitric and muriatic acid, and then subliming, he separates from the precipitate the greatest part of its copper, mercury, and osmium, and likewise of the iron which is mixed with it. A little of the remaining *platinum*, of the *palladium*, and even of the *osmium*, is removed by the same acids, because they have precipitated in the state of oxide; for in the metallic state they would not have been acted on. On the other hand, some copper and iron still remain, because they are intimately united with the other metals, and defended by them. To get rid of all the remains of the platinum, M. Vauquelin dissolves the whole again in nitro-muriatic acid, and precipitates by ammonia. He obtains a yellow and pure salt of platinum. Evaporating the residue to dryness, and treating it with water, there remains a red salt, consisting chiefly of platinum. Thus the liquid is almost completely freed from that metal. The aqueous solution is then diluted with water, and a little acid added. Some ammonia is then added, but not enough to saturate the acid; the whole is agitated; there immediately appear a great number of brilliant and beautiful red needles. They consist of the ammonio-muriate of palladium. When heated to redness, pure palladium is left behind. If a little iron and rhodium should be present, they may be separated by digesting in water slightly acidulated with muriatic acid. The residue of the liquor contains the rhodium, and some remains of palladium, copper, and iron. To obtain the rhodium, the liquid is made to crystallize. The crystals are reduced to powder, and freed from the salts of copper, iron, and palladium, by washing them repeatedly with alcohol. If any salt of platinum remain, it may be separated by means of water slightly acidulated with muriatic acid. Finally, by a last evaporation, the salt of rhodium remains of a magnificent red. When heated to redness, this salt leaves the metal in a state of purity.

We cannot by a more ingenious or simple method separate so many substances from each other, which are held together by such a powerful attraction. It is founded chiefly upon the insolubility of ammonio-muriate of palladium in water, even when acidulated with an acid; upon its precipitating as soon as it forms; and upon the insolubility of the ammonio muriate of rhodium in alcohol, while that liquid dissolves the muriates of copper and iron.

While M. Vauquelin was thus engaged in examining these two metals united to platinum, M. Laugier, his colleague in the Museum of Natural History was occupied with a third of them, and perhaps the most curious of all, *osmium*, the oxide of which is volatilized at the heat of boiling water, gives no colour to distilled water, has even the same appearance as water to the eye; but gives out a strong odour, and acts upon the olfactory nerves in such a manner as to alter the sense of smelling for several days. These properties, and several others, made chemists regret that it was so

difficult to obtain this metal in considerable quantities. M. Laugier has satisfied their wishes to a certain extent. When platinum is dissolved in nitro-muriatic acid, there remains a black powder, composed of *iridium* and *osmium*; and hitherto it was this powder only that supplied osmium to chemists. But M. Laugier, having perceived that the acid employed to dissolve platinum, when separated from it by distillation, emits a strong odour of osmium, supposed that it contained this metal: and he found that when this acid is saturated by caustic alkalies, or still better by quick-lime, and the mixture is distilled, a solution is obtained, containing a notable quantity of osmium, which formerly was entirely lost.

We spoke in 1808 of the successful trials made at Liege to obtain zinc in a malleable state in the large way, and of the advantage that might be derived from substituting it for lead as a covering for houses, and for some other purposes. An attempt was made to substitute this metal for tinned copper, and to form of it vessels for preparing food; but the Ministers of the Interior and of War having consulted the Class on the subject, the sections of chemistry and medicine found that zinc is too soluble in even the weakest acids, in fat, and even in water, and that the salts which it forms are too acrid, and in certain cases act too violently on the intestines, to allow us to employ the metals for such purposes without inconvenience. M. Sage has made some experiments on the subject, and has found that distilled water kept in vessels of zinc acquires a decided styptic taste, and that the juices of fruits, when boiled in similar vessels, dissolve a portion of them, and form a sufficient quantity of salt to render their taste disagreeable. This is a result so much the more to be lamented, as the mines from which the zinc is extracted contain no arsenic, as is the case with some others, and on that account there was no risk of any injury in the present case from that poisonous metal. We have a new proof of this in the analysis of the ore made by Sage, which he read to the Class.

MM. Vauquelin and Thenard have given an analysis of the mineral water of Provins, from which it follows that it contains in the litre (61·028 cubic inches).

Carbonate of lime	0·554
Oxide of iron	0·076
Magnesia	0·035
Manganese	0·017
Silica	0·026
Common salt	0·042
Carbonic acid	$33\frac{2}{3}$ English cubic inches.

Besides traces of muriate of lime and of a fatty matter; but it contains no sulphuric acid whatever, though the presence of that acid in it had been suspected.

M. Thenard has published the first volume of an elementary treatise on chemistry, in which that science, which daily makes so much progress, and to the advancement of which M. Thenard himself has contributed so considerably, is exhibited in its present

state. The author arranges the substances treated of according to their degree of simplicity. After having spoken of the imponderable bodies, he treats of oxygen, and of the theory of combustion. He then passes to the combustible bodies, and describes the compounds which they form with each other and with oxygen. These last are divided, according to their properties, into oxides and acids; and the fluoric and muriatic acids are placed according to the old opinions among bodies containing oxygen. Here the first part of this work stops, which the rapid progress of the science has rendered necessary so soon after other good works on the same subject; and we cannot but be anxious to see its speedy termination.

(To be continued.)

Article XVII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

London Infirmary for diseases in the eye, in Charter-house-square.

> J. B. Fane, M.D. . . . . *Physician.*
> B. Travers, Esq. . . . . } *Surgeons.*
> W. Lawrence, Esq. F. R. S. }

Farther particulars, and the terms on which gentlemen are admitted to see the practice of this infirmary, may be known on application at the house.

Dr. Spurzheim will commence his Course of Lectures on the Anatomy and Physiology of the Brain at No. 11, Rathbone-place, Oct. 18, at seven o'clock in the evening.

II. *Mistake in the Biographical Account of Scheele rectified.*

I stated, on the authority of a gentleman upon whose accuracy I placed the fullest reliance, that Retzius published Scheele's method of obtaining tartaric acid without ever mentioning his name: but I have since looked at the original paper of Retzius, in the Stockholm Transactions, and find that my statement was erroneous. Retzius particularly mentions Scheele, and states the facts which he obtained from him: so that the opinion entertained for some years, that Retzius was the discoverer of pure tartaric acid, could not have originated from a perusal of this paper. Probably it originated from the mere title of the paper, which was perhaps all that the original propagators of the story had it in their power to peruse. I consider this statement as nothing more than a piece of bare justice to Retzius, the honesty of whose conduct has been improperly called in question.

III. *Elastic Gum Bougies; and a Method of separating Ether from Water.*

(To Dr. Thomson.)

Eye, July 19, 1814.

In Number XIV. of your *Annals of Philosophy*, a Correspondent expressed a wish to know how elastic gum bougies are made ; some friend in Number XVI. communicated a method of constructing the elastic gum catheters : now there is no doubt that bougies are made in a similar manner ; but as the instruments are not alike, so the materials also differ.

I have had occasion to cut the elastic gum bougies in pieces, and the appearance of the divided part always gave me an idea that they were the catgut bougies covered with caoutchouc. To satisfy myself more of this, I prepared some ether, as directed by Mr. Winch, of London (and described in Dr. Henry's Elements of Experimental Chemistry) ; I then suspended a portion of a caoutchouc bougie in it, and after a time the gum was dissolved, leaving the catgut bare. Where, or how, they are constructed, I do not know ; but I think it likely they are made by dipping catgut bougies into a solution of the caoutchouc, in the same manner as dipped candles are made. Dr. Henry informs us, " That the method of forming tubes, &c. with this solution is described in the first volume of Fauja's Travels in England, Chap. I."

While preparing the ether for the above experiment, and the ether and water having been put into a vial so that it was about half full, I felt at a considerable loss how to separate them, until I contrived the following simple apparatus, the principle of which your readers will readily perceive. I passed a small short glass tube (whose calibre would not allow the fluid to pass) through the cork of a vial, and just even with the opposite end of the cork ; I then turned the bottle upside down, and, allowing time for all the pure ether to rise to the surface of the water, then took a red-hot poker and applied it to the *bottom* (now the upper end) of the vial, which, expanding the air, drove the water out of the bottle. This method of separating water from ether I have transmitted to you, for the purpose of communicating it to those of your readers who, like me, have not the means of getting proper chemical machines.

Should you think the above mites of information deserving a place in your *Annals of Philosophy*, the insertion of them will much oblige

Yours most respectfully,

C. B. Rose.

IV. *Diminution of Temperature from Rain.*

(To Dr. Thomson.)

SIR,

The Number of the *Annals of Philosophy* for August contained a notice respecting a remarkable fall in the thermometer which

happened during a heavy shower of rain on the 13th of May, 1813. I do not pretend to be able to solve this problem scientifically; but, as from the manner of stating it you seem to have left it open to observation, I beg leave to hazard a few conjectures by way of explanation.

The thermometer, it appears, stood at 60° about nine o'clock in the morning immediately before the shower, and while the rain was falling it sunk rapidly to 50°, and never, during the remainder of that day, regained its former elevation.

It may, I think, be remarked, that the temperature of 60° was a degree of warmth by no means inconsiderable for that period of the year, and at that hour of the morning. The rays of the sun must have exerted a powerful influence, and the surface of the earth must have been a good deal heated. That being the case, the clouds, or that portion of vapour, the subsequent condensation of which occasioned the shower, must have been suspended at too great a height in the atmosphere to produce, by the caloric given out, any very sensible change in the temperature near the surface. The immediate consequence of the shower falling is the commencement of the process of evaporation, which continues during and after the shower, till the whole, or nearly the whole, of the moisture is dissipated, and then the thermometer resumes nearly its former altitude. The effect of evaporation is the absorption of caloric and the generation of a great degree of cold, which would, under those circumstances, be rendered sensible by the thermometer. Although the power of the solar rays might be great as soon as the rain had ceased, and though the heat extricated in the upper regions of the atmosphere might extend to the surface of the earth; yet its influence is comparatively so limited, and the evaporation so considerable, that it would not be felt. It is, I believe, universally observed, that in summer, when the heat is great, a heavy shower of rain causes a reduction of temperature, or a coolness of the air; and I own I can at present imagine no way in which it can do this, excepting the one I have pointed out. At all events, the surface of the earth being heated, and the temperature of the water contained in the shower being necessarily lower than that of the earth, it follows of course that, whether there be an immediate evaporation or not, there must a part of the caloric at the surface pass into the water, and become united with it. This of itself will occasion a reduction of the sensible heat.

Upon principles nearly the reverse of the former is probably to be explained the *increase* of the temperature which generally follows a shower of rain in winter, or during a long tract of very cold weather. Then the clouds, whose condensation is to form the rain, maintain, by reason of the diminished temperature at the surface, a much less elevated position in the atmosphere. As soon as the condensation takes place, caloric is disengaged, and, from the low state of the temperature near the surface, the influence of this caloric is immediately felt, and gives rise to that augmentation of the

sensible heat which is perceived. Besides, there is now hardly any evaporation to counteract this influence. Hence we find the moisture remain so long in winter; that which is carried off being perhaps rather the product of a kind of spontaneous evaporation by the agitation of the air, than the conversion of the water into vapour by the force of caloric.

The same increase of warmth happens from a fall of snow after any continued frosty weather. The caloric given out during the congelation is immediately diffused through the atmosphere; it reaches the surface of the earth, and gives rise to that agreeable change which takes place in our sensations.

Such then is an attempt towards an explanation of these phenomena. If it be not the true and philosophical mode of accounting for them, it would, I am confident, gratify your readers if either yourself or any of your intelligent correspondents would favour the public with a satisfactory explanation of these very common occurrences.

I have the honour to be, Sir, your very obedient servant,
HENRY EDMONDSTON.

Newcastle-upon-Tyne, Sept. 4, 1814.

V. *Singular Appearance in the Eyes after Death from Hydrophobia.*

(To Dr. Thomson.)

SIR,

The following circumstance, from its singularity and importance, appears to deserve all the publicity which it can receive; and perhaps, therefore, you will oblige the scientific world by giving it a place in the *Annals of Philosophy.* It is recorded in the 12th number of the Edinburgh Medical and Surgical Journal, as one of the appearances on dissection of a man who had died of hydrophobia, and it was observed by M. P. C. Gorcy, Member of the Legion of Honour, formerly chief physician of the army, and physician of the hospital at Metz. The body was opened ten hours after death. Amongst other appearances which are detailed very minutely, he says, " But a phenomenon worthy of attention, which has not yet been observed, as far as I know, in this disease, occurred to us when examining the eyes. The iris exhibited the same motions as in life; the pupil dilated itself on covering the eye with the eye-lids, and again contracted as soon as the light was admitted. These alternate motions were as lively as during life. The colour of the iris was not changed; it was of blue gray, and had only acquired a lustre or brilliancy which might be called phosphoric. We excited this sensibility of the iris many times, and more than 12 hours after death."

Query—Upon what principles of physiology, chemistry, or optics, is such a phenomenon to be explained?

I am, Sir, your most obedient,

VI. *Society for preventing Accidents in CoalMines.*

This Society was instituted Oct. 1, 1813. It consisted in May last of the following Members :—

Patron—His Grace the Duke of Northumberland.

Vice-Patrons.

The Marquis of Bute
The Earl Percy
The Earl of Carlisle
The Viscount Barnard, M. P.
Dr. J. Cornwallis, Bishop of Litchfield and Coventry, and Dean of Durham
Sir Thomas Henry Liddle, Bart.
Sir J. E. Swinburne, Bart.
Sir Robert J. Eden, Bart.

Sir Matthew White Ridley, Bart. M. P.
The Members of the Chapter of Durham
T. R. Beaumont, Esq. M. P.
J. G. Lambton, Esq. M. P.
Cuthbert Ellison, Esq. M. P.
George Allan, Esq. M. P.
M. E. Davison, Esq.
Adam Askew, Esq.

President—Sir Ralph Milbanke, Bart.

Secretary and Treasurer—Mr. William Burn.

Permanent Committee (besides the above Members.)

The Rev. Robert Gray, D. D.
The Rev. George Stephenson, M. A.
The Rev. Thomas Baker, M. A.
The Rev. John Hodgson
The Rev. William Turner
Stephen Pemberton, M. B.
William Reid Clanny, M. D.
John Armstrong, M. D.
Henry Fearon, M. D.
J. R. Fenwick, Esq.
John James Wilkinson, Esq.
George Robinson, Esq.
Richard Pemberton, Esq.
John Douthwaite Nesham, Esq.

Thomas Hopper, Esq.
Rowland Webster, Esq.
Thomas Wilkinson, Esq.
Addison Fenwick, Esq.
Robert Surtees, Esq.
Mr. William Chapman
Mr. Stobart
Mr. John Buddle
Mr. Thomas Fenwick
Mr. Matthew Dunn
Mr. Edward Steel
Mr. Thomas Croudace
Mr. Hoyle
Mr. George Hill

Honorary Members.

Thomas Thomson, M. D. F. R. S. L. & E.
William Allan, Esq. F. R. S.

The following is a copy of a letter circulated by the President, Sir Ralph Milbanke ; dated May 4, 1814.

"SIR,

" This Society, constituted as above, and having for its object the prevention of those sudden and disastrous Explosions in Coal Mines, of which the melancholy effects have been so frequenty and recently

experienced, have to urge on the consideration of the public, that the interests of humanity are deeply concerned in its support.

" It therefore requests the favour of your countenance and co-operation, and the communication of any advice or information that may assist it in forwarding its views. The proprietors and directors of the different mines in this kingdom are particularly requested to communicate the courses and precautions now taken in their different mines to obviate the mischiefs complained of, and also the observations and amendments that may have occurred to them."

Communications and Subscriptions are received by Mr. Burn, of Southwick, near Sunderland, Durham, Secretary and Treasurer; and it is requested that Subscriptions be remitted to the Wear Bank, in Sunderland, or be named as soon as possible, as the Committee are about to publish a Report, containing a comprehensive view of the modes of ventilation now practised in the North of England; and also propose, if their funds will enable them, to offer premiums for the best treatises, or any farther important discovery or improvement, that will effectually promote the object they have in view.

VII. *Mineral Collections for Sale in Germany.*

(To Dr. Thomson.)

SIR,

I take the liberty of requesting information, through the medium of your *Annals of Philosophy*, as to the value of the enclosed notice; and, if any persons in this country have become purchasers, through what channel they procured the articles, and made the payment for them. Any other information upon the subject of specimens of minerals will be acceptable to several of your readers.

I am, Sir, your obedient servant,

London, Sept. 7, 1814.   J. C.

In the first number of the Medical and Physical Journal, published by Drs. Bradley and Willich, in March, 1799, is the following article of information.

" The student of mineralogy will learn with satisfaction, that he may be supplied from Leipzig with cabinets containing specimens of minerals arranged in systematic order, and at the following *moderate* prices.

" A collection of 150 specimens, each deposited in a separate pasteboard case, and numbered corresponding to a catalogue, in which a description is given of every piece, at six rix dollars Saxon currency, or about 1*l.* 1*s.* British. A more extensive collection, consisting of 250 specimens of superior value, furnished with a similar catalogue, and packed in drawers, in a red painted case, with lock, &c. for 20 rix dollars Saxon currency, or about 3*l.* 6*s.* 8*d.* British. And lastly, a collection of the best specimens, amounting to 500 in number, the produce of various countries of Europe, many of which are rare, and as yet non-descript, being recently collected;

and provided with a complete and instructive catalogue, &c. &c. for 60 rix dollars, or about 10*l.* 10*s.* British.

" Amateurs are requested to address themselves to Mr. Martini, bookseller in Leipzig."

The Editor of the *Annals of Philosophy* regrets that he is unable at present to give any precise information respecting the inquiry of his Correspondent. Immediately after the 1st of April he made some attempts to procure a collection of minerals from Saxony; but no satisfactory answers were returned to his letters. Indeed it is not above six weeks since the Leipsic scientific journals came into his possession, though they were ordered as early at least as the month of March; and even at present none have come to hand later than for the month of May. All this is to be ascribed to the dreadful state of confusion in which Saxony was left by the French army. Some considerable time must elapse before matters get into their usual train, and of course before there is any great probability of being able to procure mineral collections from that country.

VIII. *Bricks that Swim on Water.*

The ancients possessed the art of making bricks that swam on water. Pliny names Pitane, an Asiatic town; and Calentum and Mazilua, in Spain; as places where the materials of these bricks are found. Fabbroni some years ago discovered a substance from which similar bricks might be made. It occurs at Castel del Piano near Santa Fiora, between Tuscany and the States of the Church. The substance in question constitutes a brown earthy bed mixed with the remains of plants. Haüy gives it the name of *talc pulverulent silicifere*, and Brochant considers it as a variety of *meer schaum*. In Germany it is known by the name of *bergmehl* (*mountain meal*), in Italy by that of *latte di luna* (*moon milk*). Klaproth has lately analysed it, and found it composed of

Silica	79
Alumina	5
Oxide of iron	3
Water	12
Loss	1
	100

So that it nearly agrees with a mineral previously analysed by him, to which he gave the name of *kieselguhr*.

Article XVIII.

New Patents.

WILLIAM DONCASTER, Cavendish square, London; for a series of improvements in the construction, uses, and mode of navigating

ships and other vessels of various denominations, in marine and inland navigation; and for abstracting such powers and machinery as form an hydrostator or mill; and also a mode and combination applicable to easing the draft and accelerating the motion of carriages travelling on land; and also a dining table upon an improved principle. July 26, 1814.

George Dunnage, Hammersmith; for a method of rowing or propelling boats or any other vessels. July 26, 1814.

William Johnson, Heybridge, Essex, for an improved process of making salt. July 26, 1814.

Henry William Vanderkleft, 253, High Holborn; for a method of purifying and refining Greenland whale and seal oil. July 26, 1814.

Anthony Hill, Plymouth Ironworks, Glamorgan; for certain improvements in the melting and working of iron. July 26, 1814.

Thomas Sykes, Sheffield; for various improvements in the construction of guns, pistols, and other fire-arms; and of implements used for loading them. Aug. 4, 1814.

Sebastian Erard, Great Marlborough-street, Oxford-street, London; for improvements in musical instruments. Aug. 4, 1814.

James Collier, Upper Thornhaugh-street, London; for a machine for combing wool, hemp, flax, waste silk, cotton, hair, or any other substance or material capable of being reduced to a sliver by combing. Aug. 4, 1814.

George Courtauld, Braintree, Essex; for a spindle for the manufacture of silk thread. Aug. 4, 1814.

James Thompson, Yarmouth, Norfolk; for a method of assisting to render a ship, vessel, or craft, governable in all the cases of her motion. Aug. 4, 1814.

Jean Samuel Pauly, Little Charlotte-street, Hanover-square, London; for improvements in the construction and use of fire-arms. Aug. 4, 1814.

Article XIX.

Scientific Books in hand, or in the Press.

Dr. Spurzheim is preparing for the Press an Anatomical and Physiological Examination of the Brain, as indicative of the Faculties of the Mind. It will be illustrated by numerous interesting Engravings.

A Translation from the Swedish will shortly appear of a small but highly useful little work by Professor Berzelius, entitled, An Attempt to establish a Pure Scientific System of Mineralogy, by the application of the Electro-Chemical Theory and the Chemical Proportions.

Dr. Trotter, of Newcastle, is preparing for the Press, Reflections on the Diseases of the Poor for the last Ten Years; being a summary of the cases of upwards of 3000 patients who have received his gratuitous advice.

Mr. Forster is preparing a New Edition of his Researches about Atmospheric Phenomena, with very considerable Additions. This Edition will be illustrated by Plates.

ARTICLE XX.

METEOROLOGICAL TABLE.

———

1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
8th Mo.									
Aug. 8	W	29·62	29·61	29·615	73	50	61·5		5)
9	W	29·82	29·62	29·720	69	52	60·5		—
10	W	29·97	29·82	29·895	72	48	60·0		·13
11	W	30·05	29·97	30·010	73	56	64·5		1
12	S W	30·05	29·97	30·010	77	56	66·5		
13	S W	29·89	29·77	29·830	74	51	62·5		—
14	N W	29·76	29·74	29·750	70	49	59·5		
15	S W	29·83	29·80	29·815	70	52	61·0		●
16	S	29·75	29·75	29·750	67	46	56·5		·40
17	W	29·90	29·84	29·870	71	57	64·0		
18	W	29 98	29·92	29·950	76	53	64·5		—
19	N W	29·99	29·88	29·935	70	43	56·5		—
20	N W	29·93	29·92	29·925	70	49	59·5		
21	S W	29·87	29·77	29·820	70	53	61·5		2
22	S W	29·79	29·75	29·770	76	55	65·5		4 ☽
23	S W	29·64	29·54	29·590	79	52	65·5		—
24	N	29·57	29·40	29·485	63	55	59·0		1·21
25	S W	29·60	29·40	29·500	71	51	61·0		·28
26	N W	29·78	29·70	29·740	67	43	55·0		—
27	N W	29·87	29·85	29·860	66	43	54·5		
28	N	30·00	29·91	29·955	65	37	51·0		
29	N W	30·09	30·07	30·080	68	46	57·0		
30	W								○
31									
9th Mo.									
Sept. 1									
2									
3									
4									
5									
6									·17
		30·09	29·40	29·812	79	43	60·31		2·31

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column: A dash denotes, that the result is included in the next following observation.

REMARKS.

Eighth Month.—8. Rain last night. A stiff breeze, a. m.: rain, p. m.: a large *Nimbus* in the S. W., and a transient bow in the evening. 9. The breeze continues, with *Cumulus* and *Cumulostratus*: after a drizzling shower or two, a calm evening, with *Cirrocumulus*. 10. The night was overcast: *Cumulus*, a. m. : and *Cumulostratus*, with repeated showers, p. m. passing off very heavy to the E. at sun-set. 11. Morning, cloudy : evening, close, with a *Stratus* forming at sun-set. 12. Cobwebs on the grass with the dew : cloudy, a. m.: clear at noon : evening, calm : a fine plumose *Cirrus* in the N. 13. A few drops of rain this evening. 15. Drizzling rain : the wind veered to S. in the night. Heavy showers from nine to twelve, when it cleared up, the wind turning to the westward. 17. Fine morning, with a light wind, W. N. W. *Cirrocumulus* prevails, with other clouds. 18. *Cirrus* and *Cirrocumulus*: light showers: at sun-set the lower edges of the clouds exhibited a deep red. 19. *Cumulostratus*, with some light showers. 20. Morning, calm : much dew : the first feeling of autumnal cold. At sun-set a group of dense *Cirri* in the N. N. W., casting shadows into the atmosphere : a *Stratus* in the marshes. 21. Towards evening some light showers from low driving clouds. 22. *Cirrostratus* in the N. horizon, a. m. : and at sun-set a singular one in the W. 23. Some rain in the night: *Cumulostratus*, with showers : close and warm. 24. *Cumuli* hung like curtains round the horizon : little air stirring. *Cirri* formed above, moving briskly from the N. W., with the vane at N. Soon after eleven, a. m. it began raining, continuing to rain incessantly through the day, and at intervals through the night also. 25. A magnificent display of *Cumulostratus* this morning, followed by repeated showers: p. m. a thunder shower, the wind changing to W. 26. Some rain during the night : wind brisk and cool: *Cumulus* and *Cumulostratus*: the sky at sun-set ruddy orange, reflecting a bronze hue from the eastern clouds. 27. The night was serene. A *Stratus* this morning, with a plentiful dew : *Cirrocumulus* appeared alternately with *Cumulostratus*: a few large drops of rain at two p. m.: evening, calm : clear sun-set. 28. Morning almost cloudless : a fresh breeze from N. : much dew and cobwebs on the grass : *Cirrocumulus* in the evening, beautifully illuminated by the setting sun. 29. Misty morning : dew and cobwebs: fine day. 30. Grey morning, calm, cloudiness in the North.

RESULTS.

Prevailing Winds Westerly.

Barometer : Greatest observed elevation.. ...... 30·09 inches ;

Least .... 29·40 inches ;

Mean (of 22 days)................. 29·812 inches.

Thermometer : Greatest height .... 79°

Least... 43°

Mean (of 22 days)......................... 60·31°

Rain (in the whole period), 2·31 inches.

Evaporation not observed.

*** I am indebted for the whole of the observations contained in this report to the kindness of my brother, William Howard, by whom they were made, in my absence, at Tottenham. It appears that on the 28th ult. my thermometer rose to 92·5°.

TOTTENHAM, *Ninth Month*, 15, 1814.   L. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1814.

ARTICLE I.

*Biographical Account of Peter Jacob Hjelm, Keeper of the Mint at Stockholm.**

MR. HJELM was born on the 2d of October, 1746. He was the youngest of four children of Erik Hjelm, Curate of the Congregations of Götheryd and Traheryd, and Rector of Sunnerbo, in the county of Wexio, by a second wife, Cecilia Gistren, daughter of P. Gistrenius, Pastor in Wigeltofta.

At twelve years of age, after having received a private education at home, he was put into the Rector's Class of the Gymnasium at Wexio. He left this school in 1763, and went to Upsala. Here he followed his studies during the course of eleven years. Mineralogy was the branch of science to which he devoted his chief attention ; and in 1771 he gave the first academical specimen of his knowledge, in a dissertation on the Property of Mines and Veins. In 1774 his second thesis appeared, under the inspection of Bergman, on the White Ores of Iron. The same year he became *auscultant* in the Royal Mining College ; and two years after he was appointed inspector of the pupils. He became Assayer in the Mint in 1782, and acted as deputy to Assessor Von Engeström from that time till 1786. In 1794 he was appointed Keeper of the Mint, and Inspector of the Chemical Laboratory of the Royal College of Mines. These situations he retained till the period of his death, which took place at Stockholm, on the 7th of October, 1813.

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* Translated from the Kongl. Vetenskaps Academiens Handlingar for 1813, Part II. p. 280.

His profound knowledge had, with great propriety, attracted the attention of the Royal Academy of Sciences, of which he was elected a Member in 1784. He took his seat by delivering a discourse on the Methods employed in Chemistry, and their certainty. When, in 1787, he became President of the Academy, he delivered a discourse on Different Things which may be applied to Useful Purposes. He had several years before translated the panegyric of the Academy on the celebrated Torbern Bergman.

The 24 dissertations enumerated below, and published by the Academy in the course of 30 years, afford abundant proof of Mr. Hjelm's zeal for the sciences. His discovery of molybdenum has given him a distinguished place in the history of science; nor will his industry and assiduity as an officer and a citizen be soon forgotten.

The following is a list of the works of this industrious philosopher :—

I. *Published in the Memoirs of the Swedish Academy of Sciences.*

1. On the population of the diocese of Upsala, between 1749 and 1773. Published in the volume for 1776.

2. Experiments on the diminution of bulk of different species of trees when converted into charcoal. 1780.

3. On the constituent parts of pitcoal and charcoal. 1781.

4. Inquiry whether lime enters into the composition of sugar. 1783.

5. Experiments showing that manganese exists in brunsten (black oxide of manganese). 1785.

6. Dissertation on the nature and constituents of steel. 1787.

7. A flux useful in essaying lead ore, &c. 1787.

8. On a salt in marshberry juice (kärsbärssaft). 1788.

9. Experiments on molybdena, and on the reduction of its oxide. 1788.

10. First continuation of the preceding paper. 1789.

11. Second continuation of ditto. 1789.

12. On the quantity of oxygen gas in black oxide of manganese. 1789.

13. Third continuation of the paper on molybdenum. 1790.

14. Fourth continuation of ditto. 1790.

15. Fifth continuation of ditto. 1791.

16. Sixth continuation of ditto. 1791.

17. Seventh continuation of ditto, and conclusion. 1792.

18. Of the useful purposes to which molybdenum may be applied. 1793.

19. Method of purifying lead from a mixture of gold and silver. 1794.

20. Experiments on the spring water at Almby farm, in the parish of Gillberga, in Södermanland. 1796.

21. On the art of hardening copper. 1797.

22. Mineralogical observations on the porphyry mountains in the parish of Elfdal, in Eastern Dalecarlia. 1805.

23. Continuation and conclusion of the preceding paper. 1805.

24. Appendix to Mr. Bernde's treatise on tile burning. 1805.

II. *Published separately.*

1. Translation of Bergman's treatise on the blow-pipe. 1781.

2. Translation of Ferguson's history of civil society. 1790.

3. Observations on the best method of procuring saltpetre. 1794.

4. Instructions respecting the method of establishing saltpetre works. 1801.

5. On the preparation of indigo from the isatis tinctoria. 1801.

6. Cause of the general diffusion of the method of restoring persons apparently dead. 1791.

7. Translation of Hume's smaller œconomical essays. 1791.

8. The art of manufacturing potash. 1802.

9. Account of the Elfdal porphyry works in Eastern Dalecarlia. 1802.

ARTICLE II.

Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

I. *On the Difference between the Composition of Organic and Inorganic Bodies.*

IN different preceding memoirs I have endeavoured to unfold the laws according to which inorganic bodies combine. I have no doubt that what I have advanced on this subject will be sooner or later established by the experiments of other chemists. I mean to attempt at present to extend these laws of chemical proportions to the products of organic nature. Though this is a much more difficult task than the preceding, yet I flatter myself that I have at least ascertained the most general rules of these combinations, which in all probability art will never be able to imitate.

It is evident that the existence of determinate proportions in inorganic bodies leads to the conclusion that they exist also in organic bodies; but as the composition of organic bodies differs essentially from that of those which are inorganic, it is clear that an essential modification must exist in the application of these laws to these two different classes of bodies.

To render the perusal of the details of my experiments less dry, I shall begin by a short explanation of the conclusions which I con-

ceive may be drawn from the experiments hitherto made. This will enable the reader to judge whether the analytical results warrant my conclusions in a satisfactory manner or not.

I shall in the first place draw a comparison between the composition of inorganic and organic nature. The laws of inorganic compounds may be expressed by the two following rules :—

1. When two elementary bodies combine, they unite in such proportions that one volume of the gas of one combines with an equal volume of the gas of the other, or with two or with three volumes of that gas, without any intermediate fractions ; and in these combinations *one of the elements ought always to be considered as unity.*

According to the corpuscular theory, this means that an atom of one of the elements combines with one or more entire atoms of the other. We never find in inorganic nature combinations between three atoms of one body and four or five atoms of another. We find, on the contrary, that one of the elements exists in the state of a single atom. This is one of the circumstances which chiefly characterizes inorganic compounds.

2. When two bodies, each containing oxygen, combine, the oxygen in the one is always a multiple of that in the other by a whole number.

In all probability this law may be more generally expressed as follows :—When two bodies having a common electro-negative element combine, the electro-negative element of the one is always a multiple of that in the other by a whole number. This is the case, for example, with sulphur and arsenic in the double sulphurets and arseniurets, of which mineralogy presents us with so great a number of examples. From these two rules all the others may be deduced.

In inorganic nature compound bodies of the first order* never contain more than two elements ; so that we may say that inorganic nature *never contains more than binary combinations, and bodies composed of binary combinations.*

All inorganic bodies in which we find more than two elements are evidently composed of binary combinations of these elements, which may be separated from each other without decomposition, and generally they may be united again so as to form the compound substance anew.

The double or triple metallic sulphurets, which contain two or three metals, are always to be considered as composed of as many simple sulphurets. The alkaline sulphurets are to be considered as

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* By compounds of the *first order*, I mean bodies composed of elements, and containing no constituent part which is a compound. Thus sulphuric acid is a compound of the first order. Sulphate of potash is one of the *second*, and alum one of the *third*. For sulphate of potash is composed of two bodies of the first order, the acid and potash ; and alum of two compounds of the second order, namely, sulphate of potash and sulphate of alumina. See my memoir on the Cause of Chemical Proportions, *Annals of Philosophy*, iii. 443, &c.

compounds of a binary compound, the alkali, with an elementary body, the sulphur, and of course constitute only an apparent exception to the law.

The two circumstances, that, first, *the compounds of the first order are always binary*, and that, secondly, *in all these combinations at least one of the constituents enters only in the quantity of a single atom or volume*, constitute the exclusive characters of inorganic nature, and form what I call the *principle* of inorganic formation.

Let us now see what are the characters of organic substances. All organic bodies contain oxygen united to more than one combustible radicle; and chemical experiments on these substances have shown us that these combinations of oxygen with two or more radicles cannot be considered as composed of two or more binary bodies. Consequently organic compounds of the first order contain more than two elements. According to the number of these elements we may call them ternary, quaternary, quinquarny, &c. oxides. Thus in inorganic bodies the composition and the law of proportions are in the greatest possible state of simplicity; but in organic nature they become more and more complex, as the number of elements increases. We may compare this difference between the two kingdoms to the difference between common arithmetic and algebra.

The laws of proportions in organic nature may be comprehended under the two following general rules :—

1. When three or more elementary bodies, of which oxygen is always one, combine so as to produce a ternary, quaternary, &c. oxide, a certain number of atoms or volumes of one of the elements combines with a certain number of atoms or volumes of each of the others ; *but it is not necessary that any one of these elements should be considered as unity.*

2. When these oxides combine with each other or with binary oxides, the oxygen in the one is always a multiple by a whole number of that in the other.

This last rule is common both to organic and inorganic bodies; but the generalization of this rule as I applied it above to inorganic bodies does not apply to organic bodies, as far at least as the electropositive bodies common to both constituents are concerned. Thus in the salts formed by the combination of different vegetable acids with ammonia, we very seldom find the hydrogen in the acid a multiple or submultiple by a whole number of that in the ammonia or the water. This circumstance deserves attention.

The exclusive characters, then, of organic bodies are, that *those of the first order contain more than two elements, none of which must of necessity be considered as unity.* This is what I mean by the *principle of organic formation.*

The first of these rules would seem at first sight to indicate that determinate proportions do not exist in organic combinations, for that rule admits of combinations in all proportions ; but this by no

means excludes determinate proportions, because the ternary, &c. oxides are subjected to the same law of combination as the binary; and this must be ascribed to the same cause in organic and inorganic bodies; namely, to the existence of determinate proportions in both kingdoms of nature.

I suppose that other chemists, as well as myself, have found it difficult to conceive how such an immense variety of compounds among three or four elementary bodies could exist conformably to the laws of chemical proportions; the different species of vegetable oils, for example, of tannin, &c.; for it is to be supposed that the difference in their composition is infinitely small; but this difficulty is obviated by the single circumstance that in these ternary, &c. oxides, none of the elements is necessarily an unit; of consequence the number of possible combinations becomes almost infinite. I will endeavour to explain these circumstances by examples. It is known that when an acid combines with a base, the acid always contains two, three, &c. as many volumes of oxygen as the base; but I have endeavoured to prove (in my memoir on the Cause of Chemical Proportions,) that when an acid in these neutral salts contains three times as much oxygen as the base, the acid is composed of one volume of radicle and three of oxygen; and even supposing this not always to be the case, still the number of volumes of oxygen in the acid is divisible by the number which expresses how often the acid contains the oxygen of the base; so that if the same acid forms salts with an excess of base, it is very easy to find how many volumes of oxygen the acid contains for one volume of its radicle. For a more detailed explanation of that matter I refer the reader to my memoir already quoted.

Now if we apply these observations to the vegetable acids, we find without much difficulty the number of volumes of oxygen which they contain; and this number being once known, it is easy to determine by direct experiments what is the number of the volumes of the other elements; for example, 100 parts of tartaric acid saturate a quantity of base which contains 11·976 of oxygen. This acid (by an analysis which I shall detail below) contains 59·88 per cent. of oxygen, 36·167 of carbon, and 3·951 of hydrogen. If we state these numbers in volumes, we get 1 of carbon, $1\frac{1}{4}$ of oxygen, and $1\frac{1}{4}$ hydrogen; but the oxygen in the acid is exactly five times that in the base which it neutralizes. Hence we must suppose that the acid contains five volumes of oxygen. On this supposition the fractions disappear, and the acid is a compound of

4 volumes carbon
5 ——— oxygen
5 ——— hydrogen

100 parts of succinic acid saturate a quantity of base containing 15·975 of oxygen; but with oxide of lead, besides the neutral succinate, it forms a subsalt, in which it is combined with three times as much oxide as in the neutral combination. Hence succinic

acid ought to contain three volumes of oxygen, or 15·975 × 3 = 47·925. Accordingly analysis gives this result. It gives us, besides, 47·859 per cent. of carbon and 4·218 of hydrogen ; so that succinic acid is composed of

3 volumes oxygen
4 ———— carbon
4 ———— hydrogen

It is in this manner, by consulting not merely analysis, but by examining the compounds of which the substance is capable, that we are enabled to unfold the laws of chemical proportions in organic nature. Observations prove also clearly that in ternary and quaternary oxides we need not look among the elements for one which must of necessity be reckoned unity ; though it may sometimes happen that only a single volume of one of the elements may enter into the compound.

It is then almost impossible to determine by analysis the number of elementary volumes contained in the substance analysed. This must always be done by examining the combinations which it forms with other oxides, the composition of which is known. If a substance cannot be obtained in a state of combination, it is very difficult, if not impossible, to determine its true composition ; because when the number of volumes of one element is great, the experiment can never be made with sufficient precision to determine the exact number, unless we can find by other means the number of volumes of one of the elements.

The combustible radicles which constitute the ternary and quaternary oxides are hydrogen, carbon, and nitricum. Hydrogen and carbon constitute the ternary oxides of the vegetable kingdom. They have all less or more of the characters of acids ; that is to say, they combine with saline bases. The addition of nitricum constitutes the quaternary oxides of which the animal kingdom is chiefly composed. They have, though not without exception, the characters of bases ; that is to say, they have a disposition to combine with acids. The more complete the organization becomes, the more does the number of elements increase.

Several organic bodies contain, besides, other elements, which are just as essential to them as hydrogen and carbon ; but they enter in infinitely small proportions ; so small, indeed, that analysis cannot with any certainty determine the relation between their volumes and the number of volumes of the other elements. Such are sulphur, phosphorus (perhaps likewise the muriatic and fluoric radicles), potassium, sodium, calcium, iron, and sometimes silicon and manganese. The combustible radicles occur often in form of oxides in organic nature ; but most frequently they enter by the same manner of combination as hydrogen and carbon.

When making an analysis of blood, I proved long ago that the bodies which constitute the ashes of the colouring matter of the blood do not exist in the organic body in the same form and state as

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that which is given them by combustion. The ashes of the colouring matter of the blood consist of phosphates of lime and of iron, and some carbonate of lime; but the colouring matter itself does not contain these salts, it contains only their elements. It ought to be considered as composed of carbon, hydrogen, nitricum, phosphorus, calcium, and iron, all combined with a portion of oxygen common to them all. When the colouring matter is carbonized by heat, we obtain, not common charcoal mixed with the above-mentioned phosphates and carbonate, but a super-carburet of phosphorus, iron, and calcium, which, like all the super-carburets, is difficult to incinerate, and which is not acted on by acids. It is only by the combustion of this super-carburet that the phosphates of lime and of iron, and the carbonate of lime, are formed. It is likewise very remarkable that this super-carburet exhales during its combustion carbonate of ammonia, just as if it contained the super-carburet of the compound metallic radicle of that alkali. The composition of the melts of fishes proves the truth of what I say better than any other example. This substance, without being acid, and without containing phosphate of ammonia, gives by combustion a super-carburet of phosphorus so abounding in phosphorus, that the phosphoric acid resulting from its combustion in open vessels prevents the access of air, and puts a stop to the combustion.

It is still unknown what is the greatest number of elementary volumes which can exist together in a primary compound; but this number appears to be very great, if we can form any opinion from the small quantities of some of the fixed elements that are found in organic nature; for example, of iron in the colouring matter of the blood. Those elements which enter into organic bodies in proportions so very small I shall call *secondary elements*, though I do not mean to say that these elements are less essential to the constitution of the bodies that contain them than carbon, hydrogen, nitricum, and oxygen, which I call *primary elements*.

There is still another essential difference between organic and inorganic nature. This difference consists in the electro-chemical modification of the organic products, which does not appear to depend immediately on that which the elements have in organic nature; that is to say, on the original electrical modifications of the elementary substances. We cannot even form conjectures respecting the manner in which nature modifies the electrical properties of the elements in the economy of living bodies; nor of the means which she employs to combine the elements in other proportions, and in other numbers than in inorganic bodies.

Consequently it is impossible to determine in organic bodies from the elements and proportions of which they are composed whether they be acid or not. Benzoic acid, for example, which only contains the fifth part of its weight of oxygen, is an acid; while gum arabic and sugar, which contain nearly half their weight of oxygen, are not acids. I have not hitherto been able to perceive that either the number of volumes, or their relation to each other, determines

any thing respecting the electro-chemical properties of ternary oxides; for the saclactic acid, for example, which contains eight volumes of oxygen, is a weaker acid than the citric acid, which contains only a single volume; while the oxalic acid, which contains 18 volumes, is stronger than either of the two just mentioned. I shall show hereafter that the ratio between the oxygen and hydrogen in the ternary oxides, from which Gay-Lussac and Thenard have endeavoured to derive the properties which characterize these substances, determines in fact nothing whatever. It appears, then, that at the instant of the formation of each ternary and quaternary oxide in organic nature, its elements receive in combining a new electro-chemical modification, on which their chemical properties chiefly depend. Though we must acknowledge that the proportion of the elements ought not to be without some influence.

It appears clear that in animals nature employs the nervous system to produce these new electro-chemical modifications, and to determine the nature and composition of the various substances produced in different parts of the animal body. At the same time there are animals of so imperfect an organization, that anatomy has not been able to find in them a nervous system. Posterity must decide whether this be owing to the insufficiency of our methods to discover nerves when concealed, and probably varying in form, or to the power of nature to employ different means to arrive at the same object.

In vegetables, notwithstanding the great analogy between vegetation and its secretions and similar functions in animals, nothing has hitherto been observed similar to the nerves of animals. But if we are entitled to draw any conclusion from the strict analogy, for example, between the sexual functions of the two classes of organized bodies, we have a right to suspect that the same analogy exists between the other functions common to animals and vegetables; though it may be a very laborious task to penetrate the mystery.

In consequence of the great number of elementary volumes which enters into the composition of these ternary and quaternary oxides, and from the peculiar and special modification of the electro-chemical properties of these elements, organic bodies in general constitute but feeble compounds, which often begin to undergo decomposition as soon as they escape from the influence of the organ in which they were produced. Almost all organic bodies are decomposed by the united influence of air, water, and heat. Their elements resume their ordinary electro-chemical modifications, and there finally results a number of binary or inorganic combinations; but this seldom takes place all at once. They generally pass through a series of ternary oxides, the composition of which is varied. Thus sugar forms first alcohol, then vinegar, then a mucous matter, &c. before its organic composition is completely destroyed.

II. On the Analysis of Organic Bodies.

The analysis of organic bodies is much more difficult than that of inorganic bodies; because they are so easily altered by the situations in which it is necessary to place them, in order to obtain them pure, or in a state proper for experiment. It is often impossible to separate them from all the water of combination, the elements of which mixing with those of the substance analysed occasions an erroneous result. For these reasons I have preferred making my experiments of analysis on combinations of the ternary oxides with binary oxides, and I have almost always employed the oxide of lead, because most organic bodies have an affinity for it, and because the resulting combinations are almost always easily separated from their water of combination. I have chosen the vegetable acids as the first substances to begin with, because they constitute, as it were, the transition from inorganic to organic nature. I propose to examine by degrees some substances belonging to each class of ternary oxides, to ascertain what are the general differences between the classes; for example, between resins and oils. These differences being ascertained, it will be next necessary to examine the differences between the species themselves, as between oils of lavender, cinnamon, anise, &c.

As to the quaternary oxides, or those which contain nitricum, my method of analysis explained here is not applicable to them. Hence I shall in the first place pay attention only to the ternary oxides; convinced that it is necessary to know the more simple combinations before we begin to study the more complex ones.

It is very probable that in a course of study almost entirely new I shall sometimes commit mistakes, and draw consequences which will not be always confirmed by future researches; but I entreat the reader, who will perhaps form his opinion of me at a time when the subject will be better known and illustrated, to recollect that this was not the case when these experiments were made, and that it was scarcely in my power to profit by the information of any predecessor; but though I speak in this manner, I must, notwithstanding, do justice to those chemists who have been employed before me in the analysis of organic bodies. But among these labours we can scarcely reckon the experiments of the phlogistic chemists to decompose substances by naked distillation, in order to obtain their acid and empyreumatic spirits, their empyreumatic oils, their essential salts, gases, &c.; because they had no tendency to decide the question under our consideration.

When the theory of chemistry was changed by Lavoisier, that illustrious chemist began to consider the composition of organic bodies in a proper point of view; and he endeavoured to discover the elements of these bodies, and to determine their proportions. He discovered the nature of these elements, though he was not so successful in determining their proportions. Lavoisier's mode of analysis was to oxidize organic bodies till he converted them into

binary oxides, the composition of which was known, and to perform the experiment in such a manner that the quantity of these binary oxides produced could be determined correctly.   He burnt organic substances in oxygen gas; but this method, though just in principle, did not enable him to determine by the balance either the quantity of water or of carbonic acid gas formed by the combustion; so that his experiments could only give approximations.

Some chemists, after Lavoisier, endeavoured to determine the exact quantity of the products of dry distillation, and to calculate from them the composition of the body.   Such was the attempt of Fourcroy and Vauquelin on the composition of urea, and of Dr. Higgins on that of acetic acid.   But the composition, and probably also the quantity, of the products of distillation being only imperfectly known, it is obvious that such experiments, however exact, can only afford data for calculation hereafter, when the products of the distillation come to be examined and accurately known.

Theodore de Saussure has analysed alcohol and ether in Lavoisier's manner.   Probably the results which he obtained approach nearer the truth than any preceding analysis of a ternary oxide. Thenard has in the same way analysed different species of ether.   I cannot at present do justice to the labours of these distinguished chemists, as I have not yet employed myself in the analysis of these substances.

<div align="center">(To be continued.)</div>

<div align="center">

Article III.

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Some farther Observations on the Quantity of Carbonic Acid Gas emitted from the Lungs in Respiration, at different times, and under different circumstances.[*]   By Wm. Prout, M.D. of the Coll. of Physicians, &c.

Various circumstances have prevented me from pursuing this interesting subject in the manner I had intended.   I have, however, occasionally turned my attention to it, more especially with the view of ascertaining whether the quantity of carbonic acid gas emitted from the lungs began to increase uniformly at the same hour in the morning at all periods of the year.   The experiments, therefore, which have been made on this part of the subject, together with a few others, will constitute the chief object of this communication; which I wish to be considered merely as an appendage to my former one.

Sect. I.—*Experiments made to ascertain the period in the morning when the quantity of carbonic acid emitted from the lungs began to increase, &c.*

I mentioned in a note in my former paper on this subject that I was particularly struck with the sudden increase in the quantity of

carbonic acid emitted from the lungs in the morning about the commencement of twilight. This circumstance appeared so interesting, that I determined to ascertain, if possible, whether it were merely accidental, or whether it really obeyed the movements of the sun. With this view, therefore, I commenced a series of experiments about the winter solstice; which was continued, at intervals, nearly till the summer solstice; a period which, of course, included all the variations in the sun's rising. These experiments were made by staying up the whole of the night, and repeating the operations occasionally during that time, with the same instrument, and precisely in the same manner, as formerly described. The following are the details :—

TABLE I.

Jan. 4, 1814.
Bar. 29·1.* Ther. 37.

P. M.	Cub. In. Carb. Acid.
8ʰ 30′	3·90
10 30	4·00
12 30	4·00
2 30	3·90
4 30	3·90
5 45	3 70
6 15	4·40

Jan. 21, 1814.
Bar. 29 7.* Ther. 34.

P. M.	Cub. In. Carb. Acid.
11ʰ —′	3·30
12 —	3·30
2 —	3·30
3 —	3·30
3 50	3·00
4 45	3·00
5 50	3·55

Feb. 27, 1814.
Bar. 29·5. Ther. 38.

P. M.	Cub. In. Carb. Acid.
9ʰ —′	3·30
10 —	3·30
11 —	3·30
12 —	3·30
1 —	3·10 †
2 —	3·00 †
3 —	2·80 †
4 15	3·00
5 25	3·50

April 3, 1814.
Bar. 29·6. Ther. 50.

P. M.	Cub. In. Carb. Acid.
10ʰ —′	3·30
11 —	3·30
12 —	3·30
1 15	3·30
2 8	3·30
3 —	3·35
4 —	3·60

May 12, 1814.
Bar. 30·2. Ther. 50.

P M.	Cub. In. Carb. Acid.
9ʰ 10′	3·70
10 —	3·60
11 —	3·00
12 —	3·30
1 —	3·30
2 20	3·50

Aug. 21, 1813.
Bar. 29·8. Ther. 62.

P. M.	Cub. In. Carb. Acid.
9ʰ 30′	3·50
10 30	3·50
11 30	3·60
12 30	3·50
1 20	3·65
2 45	3·90

TABLE II.

	Commencement of Twilight.	Time of increase in quan. of C A.	Quantity of C. A. before increase.	Quantity of C. A. after increase.	Quantity of C. A. increased.
1814.	A. M.	A. M.	Cub. In.	Cub. In.	Cub. In.
Jan. 5	5ʰ 50′ ‡	6ʰ 15′	3·70	4·40	·70
—— 22	5 40	5 50	3·00	3·55	·55
Feb. 28	4 45	5 25	3·00	3·50	·50
April 4	3 30	4 —	3·35	3·60	·25
May 13	1 25	2 20	3·30	3·50	·20
1813.					
Aug. 22	2 35	2 45	3·65	3·90	·25

* The heights of the barometer are taken from meteorological tables kept in and near London, and hence are to be considered only as approximations.

† I grew very sleepy at this time. Will this account for the diminution ?

‡ These are only to be considered as approximations. They were taken from a common almanack ; and are sufficiently accurate for our present purpose.

Plate XXIV.
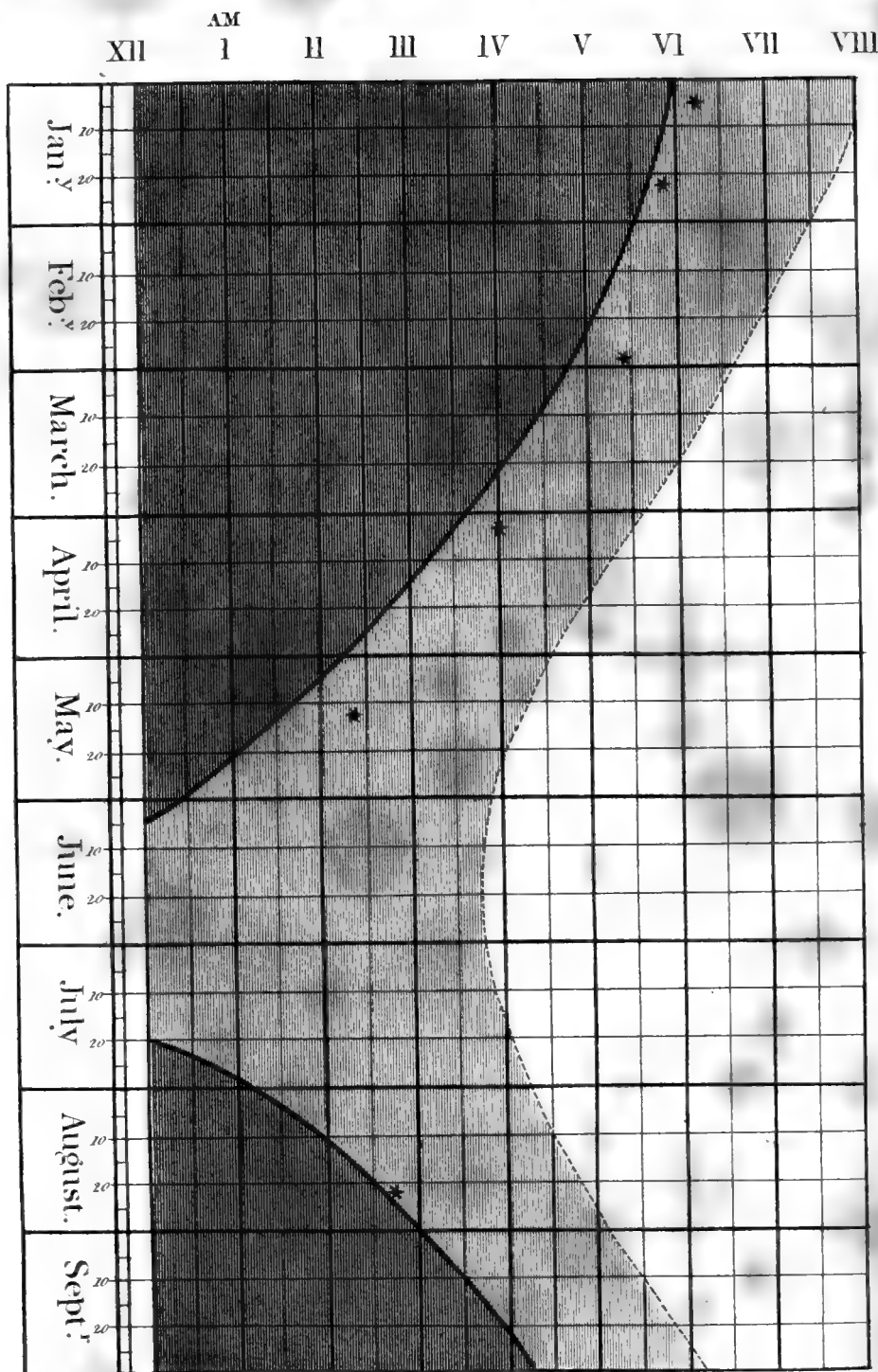
TABLE III.

Jan. 5, 1814.		Jan. 6, 1814.		Feb. 28, 1814.	
Bar. 29·2. Ther. 39.		Bar. 29·3. Ther. 36.		Bar. 29·4. Ther. 38.	
A. M.	Cub. In. Carb. Acid.	A. M.	Cub. In. Carb. Acid.	A. M.	Cub. In. Carb. Acid.
6ʰ 15′	.... 4·40	10ʰ 30′	.... 4·55	5ʰ 45′	.... 3·60
9 40	.... 4·50	11 30	.... 4·60	10 30	.... 3·40
11 30	... 4·70			12 40	.... 3·60
12 50	.... 4·30				
2 30	.... 4·30				
5 20	.... 4·10				
8 —	.... 4·00				
9 30	.... 4·00				

From the first two of these tables and the annexed diagram,
[Plate XXIV.] (in which the dark curved line represents the
commencement of twilight, the dotted line the time of the sun's
rising, and the asterisms the day and hour when the experiments,
&c. took place,) it will be seen, that *the increase has always
uniformly occurred soon after the commencement of twilight, and
before sun-rise.* It is also deserving of notice, that the increase was
greatest and most remarkable when the nights were longest, and
that it gradually became less as they grew shorter;* a circumstance
which, however, appears to have been chiefly owing to a diminution
having taken place in the usual minimum quantity towards the
morning, either probably from the fatigue of watching or from
drowsiness.

With respect to the *increase towards noon* formerly noticed, when
the quantity was found at its maximum, I have only the instances
constituting Table III. to offer in addition to what was then ad-
vanced. No exception to this part of the law has occurred to me.

The time of *the evening diminution* appears to be about the ter-
mination of twilight. I have not, however, had opportunities of
verifying this in the manner I could wish, owing chiefly to the late
hours of dining, &c. which might be supposed to interfere with
the results. Yet in Tables I. and III. some facts in favour of this
assumption may be observed : thus on Jan. 5, the minimum seems
to have occurred between five and six, *p. m.* ; on May 12, about
eleven, *p. m.* ; and in Aug. 1813, during twelve days, at a mean
about 8ʰ 38′, *p. m.* ; periods not far distant from the terminations
of twilight at these different times. In further corroboration, also,
it may be mentioned that no negative instance has occurred to me.
Hence Law I. in my former paper requires to be somewhat altered,
and instead of the periods there mentioned as the commencement

* I regret much that circumstances did not permit me to make the experiments
about the summer solstice, when the twilight in this latitude continues the whole
night. At this time it is not improbable that the quantity gradually diminishes to
midnight, and then begins to increase in a similar manner.

and termination of the minimum, the words "the ending," and "beginning of twilight," should be adopted.

Sect. II.—*Experiments made to ascertain the effects of some substances upon the quantity of carbonic acid emitted from the lungs during respiration.*

I am happy to find some of the results of my former experiments corroborated in a considerable degree by another. Dr. A. Fyfe, jun. of Edinburgh, last summer turned his attention to this subject, and unknown to me made some experiments very similar to my own, and likewise extended his researches to other objects. This I was informed of by his brother some time afterwards; and in consequence of my expressing a wish to him of knowing the results, Dr. F. was so obliging as to communicate them to me generally in a letter, dated Nov. 4, 1813; when I had the gratification of learning that, as far as we had proceeded together, we had arrived at similar conclusions. This gentleman's experiments are now published,[*] and hence I shall not scruple to mention some of their particulars.

Dr. F. fixed the standard quantity of carbonic acid gas emitted from his lungs under ordinary circumstances of health, &c. at about 8·5 per cent.[†] Having ascertained this by repeated experiments, he tried the effects of the following subjects :—

1. *Vegetable Diet.*—This reduced the quantity of carbonic acid to about 4·5 per cent.

2. *Animal Diet.*—The effects of this were various, and considerably different.

3. *Wine.*—This reduced the quantity, in one experiment, to between two and three per cent.; in another, to 5·75 per cent.

4. *Mercury.*—When the body was completely under the effects of this metal, the quantity was reduced to about three per cent.

5. *Nitric Acid.*—A course of this reduced the quantity to between six and seven per cent.

1. *Vegetable Diet.*—A course of this was commenced on the 11th of May, and continued for eight days; on the fifth day of the experiment there was no difference in the quantity of carbonic acid; on the seventh and eighth day the quantity was reduced as above stated.

2. *Animal Diet.*—A course of this was begun on the 31st of May, and continued for the same time as the above; on the fourth day the quantity of carbonic acid was seven per cent.; on the seventh and eighth days, five per cent. This experiment was re-

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[*] See Dissertatio Chemico-Physiologica Inauguralis de Copia Acidi Carbonici e Pulmonibus inter respirandum evoluti. Auctore Andrea Fyfe, jun. Edinburgi, M.DCCC.XIV.

[†] The apparatus employed by Dr. F. consisted of a vessel open at bottom, and furnished with a proper valve, which contained about 150 cubic inches. This was filled with water, and inverted in a pneumatic trough, and the air from the lungs repeatedly inspired through the valve till it was full. The proportions of carbonic acid and oxygen were then ascertained by means of Hope's eudiometer, and the assistance of lime-water and sulphuret of lime.

peated again on the 25th of November; when on the third day the quantity of carbonic acid was between six and seven per cent.; on the fourth day, seven per cent.; on the fifth day, nine* per cent.; on the sixth day, five per cent.; and on the seventh day, eight per cent.

3. *Wine.*—On the 8th of June a much greater quantity of wine than usual was taken, and the next day the quantity of carbonic acid was repeatedly found as low as above stated. On making the experiment again with less wine, the quantity of carbonic acid was considerably reduced, though not so much as before. These experiments of Dr. F.'s differ from mine formerly stated, inasmuch as his were not made till the *day after* the wine had been taken, whereas mine were made immediately, and whilst its first effects were present.

4. *Mercury.*—The experiment with this metal was made upon a man labouring under syphilis, and who had been taking it for some weeks for that disease. In consequence of the above-mentioned great diminution observed by Dr. F., and which he noticed to me in the letter just mentioned, I was induced to ask permission of Mr. Astley Cooper to make the experiment upon some patient of his in Guy's Hospital who might be fully under its effects. This he very kindly consented to ; and the following are the details of my observations :—

1814.	A. M.	Bar.	Ther.	Pulse.	Quantity of C. A. per cent.
Jan. 3	12ʰ —′	29·7	36	—	3·95
4	12 —	29·2	35	64	2·80
6	12 —	29·3	36	60	3·80
19	10 30	29·1	33	—	4·30
20	11 —	29·5	34	70	4·50
25	12 30	29·9	—	72	4·30

The subject of these experiments was a young man about 20 years of age. He had been under the effects of mercury for four or five days. The first three experiments were made at the hospital, the last three at my apartments after he had been dismissed from it cured. There is a considerable diminution in the quantity of carbonic acid, but apparently not so great as in Dr. F.'s experiment. Dr. F., however, did not ascertain the quantity of carbonic acid

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* This is a striking illustration of Law II. stated in my former paper ; namely, when the quantity has been raised above the standard, it is subsequently depressed below it. I find also that the barometer sunk from 29·85 to 29·50 on this day. I had suspected this, and was induced in consequence to examine the meteorological journals. It is very singular, but in every instance I have uniformly found any remarkable increase in the quantity of carbonic acid accompanied by a sinking barometer. Consult the tables in my last and present paper.

emitted under ordinary circumstances by the person on whom he operated.

5. *Nitric Acid.*—The experiments with this acid were made in June. Dr. F. commenced by taking f ℥ ss. of it diluted with water, and afterwards gradually increased the dose ; on the third day of the experiment the quantity of carbonic acid was seven per cent. ; on the sixth day, between six and seven per cent., as above stated. On repeating also the experiment in August, nearly the same results were obtained.

Such is a short account of Dr. Fyfe's and my own experiments. Perhaps it will not be amiss to close the subject with a few general observations.

With respect to the numbers here brought forward, as well as those in my former paper, I wish it to be distinctly understood that they do not represent the measures of the quantity of carbonic acid emitted *in* any given time from the lungs, but the measures of the power or capability of the lungs *at* any given time, to form or throw off carbonic acid. This was not so distinctly pointed out in my former communication as it ought to have been. What I mean then to say is this, that the *power* or *capability* of the lungs for forming and throwing off carbonic acid is *greater* at noon, &c. ; and not that a greater quantity of it *is actually thrown off* at that time than at any other. A greater quantity will indeed be thrown off, cæteris paribus, on an equal number of similar respirations being made in the same given time. But whether this be really the case or not, I cannot pretend to determine.

If the hypothesis which was formerly advanced be founded in truth, it is evident that we have in these numbers a sort of relative measure of the degree of energy of the nervous system; but whether it be a measure of the energy of this system as it operates through the medium of the blood, or the muscular apparatus of circulation or respiration, or partly on all the three, I am unable to decide ; that is to say, I do not precisely know whether the changes under consideration are to be ascribed to differences in the state of the blood itself, or to differences in the state of circulation or respiration. Perhaps all these causes may contribute in some degree to produce them.

It is true that Dr. Fyfe did not observe at all these diurnal variations. But this has little or no weight, when the apparatus he employed be considered ; since it was incapable of showing it, or at least the variations would be so extremely small that they would most likely escape observation. For my own part, I do not consider either my own experiments, or his, entirely conclusive on any one point. This is a thing that can hardly be settled by one or two observers. Indeed nothing but the striking uniformity that has occurred to me, and the little probability at present of my being able to repeat them in a less exceptionable manner, added to my wish of correcting as above what I formerly advanced, would have

induced me to submit these observations to the world. Hence I content myself with little more than stating matters as I have found them, still waving all speculations, however interesting they may be, till the matter has been thoroughly investigated, or at least much better examined into, either by myself or others.

ARTICLE IV.

A Geognostical Sketch of the Counties of Northumberland, Durham, and Part of Cumberland. By Thomas Thomson, M.D. F.R.S. With a Map.*

THE counties of Northumberland and Durham (including a portion of Cumberland and of Yorkshire) in a mineralogical point of view, are perhaps the most important in Great Britain ; since they supply London, and almost all the east and south coast of the island, with coals; and since the quantity of lead which they annually yield is probably not much inferior to what is furnished by all the rest of Europe. These counties have been mined in all directions for so long a series of years, that we are perhaps as well acquainted with their internal structure as with the nature of the surface. Most of the beds extend with great regularity for a considerable space. They have been recognized by the miners as the same, and have been distinguished by the same names at places situated at a considerable distance from each other.

The facts from which I have been enabled to draw up the following sketch were ascertained by me during a tour of a few weeks duration, which I made last August, on purpose to form some notion of the nature of these important counties. I traversed both Northumberland and Durham in various directions ; and partly from my own observations, partly from the information which I obtained from the different mining agents, I was enabled to form what I conceive to be a pretty accurate idea of their structure. To assist the reader as much as possible in following the description which I mean to give, I shall insert an outline map of the counties, on which I have coloured the different formations in order to distinguish them from each other : but I must warn the reader not to suppose that the boundaries of these different formations have been marked out with precision. In some few spots I succeeded myself in determining the termination of a formation with tolerable exactness. In many cases I have been guided by the information of the mining agents; but I have no doubt that I have committed occasional mistakes, especially respecting the north-west part of Northumberland, which I did not visit ; nor did I ascertain the structure of the Cheviots, to what class of rocks they belong, or

* The Map will be given in the next number.

whether they lie over or under the lowest Northumberland formation with which I am acquainted. I am likewise uncertain about the north side of the Tees, near the sea, which I did not visit, and therefore leave the formations about that place unfilled up in the map, though I think it not unlikely that the magnesian lime-stone extends as far as the Tees, and even crosses into Yorkshire.

The counties of Durham and Northumberland form the northernmost part of England, and constitute a kind of triangle, having its apex at Berwick-upon-Tweed, and its base upon the river Tees, which separates Durham from Yorkshire. On the east it is bounded by the sea; on the north, by the river Tweed; on the west, by Scotland and the county of Cumberland; and on the south, by Yorkshire. Its greatest length, from north to south, is about 88 miles; while its greatest breadth, from east to west, reckoning from Crossfell (which, though in Cumberland, is a part of the Northumberland and Durham formations,) to Hartlepool, is about 52 miles. The whole east side of these counties is low, and tolerably level; though here and there we meet with hills, especially in the neighbourhood of Sunderland, and towards the north. But the height above the sea increases as we proceed westward till we come to the top of Crossfell, which is one of the highest mountains in England, being elevated about 2901 feet * above the level of the sea: yet so gentle and gradual is the elevation on the east side, that one may ride on horseback to its very summit. On the west side it is much more precipitous, rising abruptly from the vale of the Eden in Westmorland and Cumberland.

The whole of this space, as far as my observations extend, is occupied by three distinct formations; one of which is very widely distributed; but the other two, as far as we know at present, are peculiar to Northumberland and Durham. These three are the Independent Coal Formation, the Newcastle Coal Formation, and the Magnesian Lime-stone Formation. The first of these constitutes the basis of the two counties. It terminates westward with Crossfell, in Cumberland; seems to occupy the whole of Durham; and constitutes, I presume, the whole of that part of Northumberland which lies on the east side of the Cheviots. I observed it in the vale of Wooler, at Tweedmouth, and at Belford; and I believe that coals are found almost all the way between Belford and Morpeth. It extends a little way to the north of the Tweed, but disappears almost immediately, being covered in the neighbourhood of Berwick, and on the north side of it, by the newest floetz trap rocks; nor does the same formation make its appearance again till we approach the town of Tranent, in East Lothian.

I. This Independent Coal Formation is composed of beds that lie one above another, and extend over the whole counties. They dip to the east, and crop out towards the west, till at last the lowest of them makes its appearance at Crossfell, lying immediately over the

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* This is the height given by Col. Mudge. Preceding observers had made this mountain as high as 3390 feet.

old red sand-stone, which constitutes so great a portion of the surface of Westmorland and Cumberland, and which extends as far south as the town of Manchester; part of which, as may be seen distinctly on the banks of the river that separates Manchester from Salford, is built on this rock. From this position of the beds, and from the numerous veins containing lead ore which traverse them in different directions, and which have been wrought to a considerable extent for many years, all the different beds constituting this formation have been reckoned; and their position, thickness, and nature, ascertained with much precision. They amount (confounding together several insignificant beds composed nearly of the same materials) to 147, constituting a thickness (supposing them all above each other, as is the case at the east side of the counties,) of 459 fathoms, and consisting of four different kinds of mineral, which alternate with each other a number of times : these are *coal*, *lime-stone*, *slate-clay*, and *sand-stone*.

1. I do not know exactly how many beds of coal exist in this formation; but the best information I could procure makes them amount to seven, none of which exceeds three or four feet in thickness.* The coal is usually slate-coal, similar to the Scotch, Welsh, or Staffordshire coal, and does not cake. The lowest bed of all, which crops out near Crossfell, is called crow-coal by the country people. It falls to powder when exposed to the air, and cannot be burnt by itself; but the common people make it up into balls with clay, and use it for fuel. This fact refutes a vulgar notion, which I have heard repeated by various persons, that the goodness of Newcastle coal is owing to its depth; for this bed of crow-coal lies 387 fathoms below the lowest of the Newcastle beds, and must therefore have been deposited long before them. None of the coals raised from the Independent Coal Formation in Northumberland and Durham are carried to London, or indeed sent to sea. They are all employed for the home consumption of the counties. They are considered as inferior in quality to the Newcastle coal.

2. There are 19 beds of lime-stone, situated at a considerable distance below each other, and varying very much from each other in thickness; some not exceeding the thickness of three feet, while others amount to more than 10 or 12 fathoms. This lime-stone is usually hard, has a blue colour, and is not the least crystalline in its texture. In general it is destitute of petrifactions; but there are two of the beds which constitute an exception to this rule. The first is a lime-stone, so hard as to take a polish. It is quite filled with madrepores. Unluckily I neglected to note down upon the spot the name of this bed; but, unless my memory deceives me, it is called Tyne Bottom Lime-stone; because it forms the bottom of the south Tyne, near Alstone. If this be the name of the bed,

* The overseers of the coal-mines in these counties are very different in their behaviour from the agents of the lead-mines. They affect mystery and concealment. Hence one is afraid to depend upon the information which they communicate.

it constitutes the 11th lime-stone bed, reckoning from the surface, and is about four fathoms thick. I am certain at least that it was at no great distance from Alston that I met with this bed. The second exception is a thin bed of limestone (the ninth bed from the surface), not a yard thick, quite full of shells, and known by the name of Cockle-shell Lime-stone.

But the three best known lime-stone beds are called the Scar Lime-stone, Little Lime-stone, and Great Lime-stone. The Scar Lime-stone constitutes the roof of the level driven in the direction of the river Nent, from the town of Alstone, towards Nent Head. This level has been cut at the expense of Greenwich Hospital, to which that tract of country belongs, and has been working these 30 years past. It has now advanced about five miles; but has still several miles farther to go. I went up this level nearly to its head, and learned more respecting the directions, and relative ages of the veins that traverse these beds than I could have done by visiting perhaps all the lead-mines in the district. The Scar Lime-stone constitutes the eighth lime-stone bed, reckoning from the surface. It is a hard blue lime-stone, about five fathoms thick. The word *scar* in this part of the country is applied to a *rock*. Hence probably the origin of the term.

The Little Lime-stone is the 13th bed of lime-stone, reckoning from the surface; and is so called to distinguish it from another bed, which lies at some distance below it, constituting the 16th bed of lime-stone, and is called the Great Lime-stone, because it is the thickest bed in the formation. The thickness of the Little Lime-stone bed is about three fathoms; that of the Great, about twelve fathoms. The lime-stone in both is blue, hard, and destitute of petrifactions. These two beds, especially the last, interest the managers of the lead-mines in these districts more than any of the others, for a reason that will soon appear.

3. The rock called *slate clay* by mineralogists is distinguished in this district by the name of *plate*, doubtless because it is composed of plates lying above one another. The beds of slate-clay are very numerous, amounting, as far as I could make out, to above 60. They are mostly thinner than the beds of the other minerals, those of coal excepted. This thickness seldom exceeds five or six fathoms, and is usually much less. I was told indeed of one very thick bed of slate-clay; but I did not see it. It was not far from the lowest in the series of beds. This slate-clay varies very much in hardness in different beds; sometimes it is so soft as to crumble readily to powder on exposure; sometimes so hard as to require being blasted by gunpowder. In this last state it is called *whin* by the miners. When I first heard the term *whin-stone* in Northumberland, I naturally applied to it the meaning which it bears in the south of Scotland, and conceived that it signified *green-stone*, *basalt*, or *porphyry-slate*. But the sight of a few specimens of *whin* from the miners soon convinced me that I was mistaken. The term is not limited to any particular species of rock, but alludes solely to the hardness. It is applied indifferently to sand-stone and

slate-clay whenever they are so hard as not to be capable of being wrought, except by blasting. All the specimens of *whin* shown me belonged to one or other of these two rocks, and I saw no other mineral except these to which the term was applied.

The colour of the slate-clay varies a good deal in different beds. Sometimes it is black, bituminous, and breaks spontaneously into thin slaty fragments. This variety, which approaches the nature of shale, is called *shiver* by the miners. The most common colours of the slate-clay are blue, brown, and grey.

4. The sand-stone beds amount to about 50. Many of them are thin, scarcely exceeding a fathom; but there are some beds, especially near the bottom of the formation, nearly 18 fathoms thick. This sand-stone is usually fine-grained, and has commonly a grey or yellowish colour. It is distinguished among the miners by the name of *hazle*, obviously from its colour. When the grains are uncommonly large, as is the case in some of the beds, it is called *grit*; and some beds, from the uses to which they are applied, have received particular names, as *mill-stone grit, grind-stone sill*. The word *sill* is the term applied in this country to a mineral *bed*. One of the most beautiful sand-stone beds is the one which constitutes the summit of Crossfell. It is fine-grained, of a light grey colour, pretty hard, and seems very well adapted for a building stone. I conceive it to be the sixth bed immediately below the Great Lime-stone; but I might easily be mistaken, as there was a considerable interval between the place where the Great Lime-stone was exposed and the summit of Crossfell; and this interval, being covered with moss, could not be examined.

5. This formation is intersected by a great variety of veins, most of which are very well known, as they have been wrought as mines for at least these 200 years. The oldest of these veins runs from north to south; but as they are the least numerous, and most insignificant, they are called by the miners *cross veins*. In going up the level from Alstone towards Nent Head, I had an opportunity of seeing a variety of veins cross each other, and of ascertaining that the north and south veins are always the oldest. But the most numerous and important set of veins are those that run east and west. They are the newest; but as they contain most of the lead ore which has given importance to this district, they are considered as the principal ones. These veins differ very much in wideness; some are only a few inches wide, while others are several fathoms. The width of the same vein is found to vary in the different beds through which it passes. It is apt to become narrow in passing through the slate-clay beds, and to widen very much when it passes through the lime-stone. Such a widening is called *flats* by the miners. I saw a striking example of it at Allonheads mine. The vein before it entered the Great Lime-stone was two or three feet wide, and very poor of galena; but on entering the Great Lime-stone it suddenly swelled out to a width of twelve fathoms and became exceedingly rich in galena. This increase of produce is found to be almost a constant attendant of this swelling out. Hence

the great importance attached to the lime-stone beds in this country. A very great proportion of the lead ore has been extracted from the Great Lime-stone. Every mine I visited situated in the Great Lime-stone was exceedingly rich. This was the case at Crossfell, Allonheads, and some of the mines in Weardale. One of the richest mines for many years was Rampgill, situated in a hill between Garrigill Gate and Nent Head. The vein runs east and west, and in the Great Lime-stone became of considerable width, and consisted in a great measure of pure galena. This mine is still considered as good, though it is now greatly inferior to what it once was.

The substance with which these veins are filled is frequently quartz. The vein called Crossgill furnishes one of the most striking examples. It is a north and south vein, and passes along the east side of Crossfell. As it stands above the surface, it is very conspicuous. I traced it for some miles. It is about a fathom wide, and consists of pure white quartz. Here and there pieces of iron and copper pyrites are found in it, especially near the banks of a small rivulet which crosses it. A level was driven in here for some way, in order to ascertain whether the quantity of copper was sufficient to make it worth while to work the vein; but the experiment was not attended with success.

Another mineral very common as a vein-stone in this county is lime-stone. In many mines I saw a considerable mixture of clay in the stuff brought up from the vein; and hence am disposed to consider clay as another substance with which these veins are occasionally filled. Fluor spar is another common vein-stone, and occurs more or less in almost all the veins that contain lead ore. In Weardale (that is to say, the mining district on the banks of the Weare) most of the veins that I examined were filled with fluor spar and galena.

Galena is the substance to which these veins owe their value. Sometimes it constitutes a thin bed (if I can use the expression) in the centre of the vein; but more frequently it is irregularly dispersed through the vein-stone. The poverty or richness of a vein always refers to the proportion of galena which it contains. Veins are usually richest in lime-stone, and poorest in sand-stone and slate-clay. They have been found to become the poorer the deeper they are followed. Just the opposite of this is the case with the copper-mines in Cornwall, which become more and more productive the deeper down they are followed.

The fluor spar is usually crystallized in cubes, and some of these cubes are of an enormous size. I have seen them four or five inches a side, and probably they have occurred still larger. At present they are much scarcer than they used to be. In some of the mines (at Allonheads, for example, where I saw very perfect examples *in situ*) whose vein-stones consist chiefly of a mixture of fluor spar and quartz, a very unaccountable circumstance has been noticed lately. The fluor spar has in many places totally or partially disappeared, leaving a cubic cavity in the place which was formerly occupied by the fluor crystal. In what manner this singular

removal of the fluor spar is to be accounted for I cannot conceive. The only substances present were fluor spar, quartz, and galena. The change took place in the centre of a solid vein, a hundred fathoms below the surface of the earth, surrounded on all sides by walls of solid stone, and quite impervious both to air and moisture. There cannot be the slightest doubt that the fluor spar once existed, and filled up these cavities. By what agent has it been removed, and what has become of it? This problem is well worth the consideration of geologists. It shows us that we are much farther from an accurate knowledge of the constituents of bodies than is generally believed, and that several of our elements are undoubtedly compounds. It shows us also that changes are still going on in the internal parts of the earth which are totally beyond our comprehension.

I may mention another appearance equally unaccountable, which I observed in some of the lead-mines on the banks of the Weare. The galena has lost its usual form, and has all the appearance of having been recently in a state of fusion. Its surface has a glassy lustre, its corners are rounded, and it is suspended like icicles in the cavities of the vein. This change must have happened long after the formation and filling up of the vein, and long after every thing had assumed nearly its present state. We know of no other agent to which this fusion can be ascribed than electricity; and as we know nothing respecting the limits of this agent, it is employed without scruple by geologists to solve similar appearances.

The lead-mines at present working in these counties cannot be fewer than 100. The veins were first discovered in the beds of the rivers of the country, the Tyne, the Nent, the Allen, the Weare, and the Tees. Accordingly it is on the banks of these rivers, and hills between them, that most of the mines are situated; but as I did not visit the mines on the banks of the Tees, I had no opportunity of acquiring much information respecting them.

Besides the minerals already mentioned, there are several others found in these veins. One of the most common is blende, the never-failing companion of galena. The blende is always brown, and its quantity, at least in the veins which I examined, is not great. But I was told of a vein of considerable thickness filled with blende, lately discovered in the tract of country belonging to the Greenwich Hospital; and I understand there is an intention of raising this ore, and extracting zinc from it. Calamine sometimes occurs, but it is scarce, except in one mine at Nent Head, where I saw a considerable quantity of it, and understood that it was occasionally collected and sold. Chalcedony occurs likewise in some veins, not in any great quantity, except in two very poor mines near Blanchland, called Ramshaw and Shieldon. These mines were formerly very rich, and were wrought for many years with great profit by the British Lead Mining Company. But after the veins had penetrated through the Great Lime-stone into sandstone, they became very poor, and were in consequence neglected.

They have been wrought for some years by a Newcastle company, I should conceive at an immense loss. These were the only lead-mines in the country in which I saw steam-engines employed. At all the others the machinery is driven by water.

There is a very remarkable north and south vein, which crosses the country a little to the west of Allenheads. I did not myself see it, but obtained my information respecting it from Mr. Emersom, one of Colonel Beaumont's principal agents at the Weardale mines. It causes the beds on the west side to dip about 40 fathoms ; and, what is remarkable, this dip is not instantaneous, but gradual. The beds slope down on the west side till they become 40 fathoms lower than they are on the east side.

Of the cubic galena, of which the lead ore here consists, there are two varieties. The first, which is uncommon here, breaks into large cubes with a smooth splendent surface ; the second, which is almost universal, is apt to break into small cubes, the surfaces of which are not flat, but waving, and the lustre is inferior. I was informed by Mr. James Mulcaster, who has the charge of the smelting-house belonging to the Greenwich Hospital, and who has made many experiments on the subject, that the first of these varieties always yields more lead than the second. It would be worth while to examine whether the second variety contains any antimony, or similar foreign body, which makes its escape during the process.

According to the best information which I could procure, the quantity of lead ore raised annually in this country (not reckoning the Teesdale mines) amounts to 67,500 bings, or 540,000 cwt. ; for a bing of ore weighs eight cwt. Now eight cwt. of ore yield at an average $5\frac{1}{4}$ cwt. of lead.* Thus the whole of the lead annually brought into commerce from the neighbourhood of Newcastle amounts to 354,375 cwt. Were we to reckon this only at 30*l.* the fother (of 21 cwt.), the annual value would exceed half a million sterling.† About one third of this enormous quantity of ore is raised by Colonel Beaumont, to whom a great number of most valuable mines belong, and who is the greatest miner in this county, and perhaps in Great Britain, or even in Europe. The British Lead Mining Company come next after Colonel Beaumont.

* Pure galena is a compound of 25·974 lead and 4 sulphur. Hence eight cwt. of galena contain 6·94 lead. But the galena employed in the smelting houses is never perfectly pure, containing always antimony and iron, besides portions of fluor spar and quartz. This at least I found to be the case in all the specimens I examined. Hence the 2¼ per cent. of loss, which appears to take place in smelting the ore, is probably chiefly owing to the presence of these foreign bodies. Yet I think it probable that some improvements might be introduced into the mode of roasting the ore, where I suspect the greatest loss of lead takes place.

† I have omitted introducing into this statement the Teesdale lead-mines, which are about 50 in number, because I had no information respecting them. Should they raise one-third of the ore furnished by the other parts of Durham and Northumberland, which is not improbable, the whole lead furnished by this district would amount to 531,562 cwt. annually, the value of which would exceed three quarters of a million sterling.

The remaining mines are parcelled out among various companies of miners. The valuable estates of Lord Derwentwater belong to Greenwich Hospital. They do not work any of their mines, but receive one fifth of the ore raised in them as rent. This amounts to between 3000 and 4000 bings of ore in a year.

The quantity of silver extracted annually by Col. Beaumont from his lead amounts to 15,000 ounces ; so that the quantity of silver annually raised in this county may be reckoned at about 3,750 lbs. troy.

6. Great improvements have been of late years made in the method of smelting the ore. The consequence has been, that the produce of lead has been considerably increased. The galena reduced to small pieces is in the first place roasted in a flat oven-shaped furnace by means of a reverberatory fire. It is kept moderately red-hot, and is very frequently stirred up. Care is taken to prevent the heat from rising so high as to melt the galena, and the process is stopped when the ore begins to soften. This process renders the galena darker coloured. A white smoke is exhaling during the whole process, but I could not perceive in it the smell of sulphurous acid. The smoke, I believe, is chiefly oxide of antimony, which carries with it a portion of oxide of lead, and no doubt the sulphur with which these metals were combined is acidified, though I did not perceive any distinct indications of this by the smell. This matter is collected in long horizontal chimnies constructed on purpose, and is known in the smelting-houses by the name of lead fume. It is sold as a paint. I had the curiosity to examine a quantity of it. Its specific gravity was 5·882, and I found it a mixture of about five parts carbonate of lead and three parts oxide of antimony, not reckoning a little water and earthy matter, which was probably only accidentally present. We see from this that a portion of the lead flies off during the roasting. Perhaps this loss might be somewhat diminished by keeping the fire as low as possible consistently with the object in view. It is probably the antimony that facilitates the escape of the lead in this process. This circumstance suggests a reason why the specular galena yields more lead than the variety with a waved surface. The first contains no antimony, and probably sustains in consequence less loss of lead during the roasting. The roasted galena I find unaltered in its properties. It is still as before the process composed of 25·974 lead and 4 sulphur. Hence the only use of the roasting is to drive off the antimony.

The roasted ore is then thrown along with pit-coal upon an open hearth, and the fire is kept up by means of a pair of bellows driven by water. Some peat is always put before the nozzle of the bellows to divide the blast. A little lime is also occasionally thrown in, the object of which is to prevent the slag from running away in a liquid state. In this hearth the lead is reduced to the metallic state, and drops down into a cavity in the bottom of the hearth. When this cavity is once full, the lead runs along a channel cut for it into an

iron pot, from which it is ladled out, and cast into pigs of a determinate weight.

A considerable quantity of metallic lead remains entangled in the slag. To obtain this the slag is put into a blast furnace, and urged by a heat sufficient to bring it into perfect fusion. The lead now falls down through the liquid, and is collected at the bottom of the furnace. The liquid slag is run into a reservoir of water, where it breaks into a black powder, having all the appearance of black glass. I analysed a quantity of this black glass, and found it a compound of silica, lime, and oxide of iron, with some alumina, oxide of lead, and oxide of antimony. I think it unnecessary to state the proportions, because they must vary according to circumstances. The earthy matters were no doubt partly mixed with the ore, and partly contained in the pit-coal employed to reduce the galena. The pit-coal is probably the matter that furnishes the iron, though some pyrites may be occasionally present in the ore. I forgot to mention it as a substance occasionally met with in the veins.

I found the specific gravity of this black slag 3·225. When mixed with lime it forms an excellent water mortar, and is sold for that purpose to a considerable amount. As the mortar dries it becomes yellow. I should conceive this to be owing to some oxide of lead which the slag contains, and which the lime gradually separates.

This black slag is again searched for lead, and the quantity extracted from it more than repays the expense of the process.

The lead, when the proportion of silver which it contains will cover the expense, is converted into litharge by exposure to heat upon a test. The litharge is blown off, and the silver remains behind. The litharge is again melted in a furnace with common pit-coal, which speedily reduces it to the state of lead. When thus treated it is known in commerce by the name of *refined lead*, and is the purest and most valuable sort of that metal.

(*To be continued.*)

Article V.

Observations on the Uses of the Dorsal Vessel, or on the Influence which the Heart exercises in the Organization of articulated Animals, and on the Changes which that Organization experiences when the Heart or the Organ of Circulation ceases to exist. By M. Marcel de Serres.*

Of all the questions which remain to be solved in the anatomy of animals without vertebræ, there are certainly few more interesting

* Translated from the Magasin Encyclopedique, iii. 10. May, 1814.

than that which has for its object to determine the influence which
the heart exercises on their organization, and the changes which
that organization undergoes when the heart, or organ of circula-
tion, comes to be wanting; but, to determine with precision this
double influence of the presence or absence of the heart, we must
attend to those animals in which we see the transition from those
which have a heart to those which have only vestiges of it; and
likewise to the corresponding transition from those which breathe by
circumscribed organs to those in which the tracheæ distribute air
over every part. Articulated animals being the only ones that offer
these two systems of organization, to them alone we have directed
our researches. When we study in a general manner the organiza-
tion of articulated animals, we soon perceive how many modifica-
tions their heart undergoes before it is replaced by the organ known
in insects by the name of the *dorsal vessel*, which has nothing in
common with a heart but its position. Thus in the *anelides* the
heart is in some respects double; at least we do not perceive a single
organ of circulation, but only a swelling in the two principal
vessels, which, situated the one below the other, produce the circu-
lation. The elongated form of these vessels shows that the heart will
speedily undergo greater modifications. If these modifications are
not yet very sensible in the crustaceous decapodes in which the
heart is rounded, they become very apparent in the crustaceous
brachiopodes, in which that organ assumes an elongated figure little
different from the dorsal vessel in insects. In short, as the organi-
zation simplifies, the heart assumes a greater analogy with the
dorsal vessel, both in its form and position, as is observable in the
organ of circulation of the arachnides. The heart is reduced to a
simple longitudinal vessel, which scarcely exhibits any sensible
swelling. The first of the articulated animals, the arachnides,
exhibit respiratory organs, which receive air by stigmatiform open-
ings placed on their external face. These organs become more
numerous, though they are circumscribed. This ought to be the
case, because the blood in these animals, having a true circulation,
is contained in particular vessels which carry it to the respiratory
organs, where it receives air.

The case is very different in insects. The respiratory organs,
instead of being circumscribed, are formed of elastic tracheæ,
which distribute air over every part. In them there is no heart
whatever, and the blood is no longer contained in particular canals.
A simple vessel without vascular ramifications occupies the place of
a heart, of which it may be considered as a vestige, or, if the idea
is preferred, which it replaces; but its uses are no longer the same,
and have nothing analogous.

It is the functions of this vessel that we must determine. This is
one of the objects to which we have directed our chief attention.
We have varied all the experiments respecting it which it was pos-
sible to make. This has enabled us to throw some light on one of
the most difficult parts of the anatomy of articulated animals,

respecting which, notwithstanding the fine experiments of Lyonnet, Swammerdam, Comparetti, and Cuvier, naturalists had not made up their minds. The more progress we make in the details of the organization of articulated animals, the more we see the justice of the fine theorem explained by the illustrious author of the Comparative Anatomy, that whenever the blood does not go in search of air, the air must come in search of the blood. The insects are undoubtedly the articulated animals that give the most evident demonstration of this general law, as they are likewise the animals in which air plays the greatest part. This fluid is spread through them in so great quantity, that we may say with propriety that every part of an insect breathes or enjoys the impression of the air, that subtile fluid, which, according to the fine experiments of the ancients, is as well the aliment of life as of flame.

Insects appear at the head of articulated animals with respect to the quantity of their respiration. They owe this advantage to the absence of vessels destined to contain the blood, and to the necessity in which they are of having a circulation of air, since they have no circulation of blood : but how the circulation of this incompressible fluid is produced, and by what vessels it is performed, are questions which it is of importance to inquire into and decide. It may take place in the same manner for those who respire air unchanged as for those who separate it from water. Considered in this point of view, our observations may have some interest. No observer before us had described in a general manner, and with the necessary details, the variations which the respiratory organs in that order of animals experience, nor the differences observable in them which depend upon the mode of respiration itself. We have not been less careful in examining the other details of the organization. The great number of observations which we have made on articulated animals has demonstrated to us the influence which the heart exerts over the absorbing system. Accordingly we have found the chyliferous vessels follow in some measure the blood-vessels, and never show themselves without the last, though in some cases they disappear before them. This at least is what we have observed in the arachnides, the last class of articulated animals, in which there exists a heart and blood-vessels. This connection had been perceived before. M. Cuvier, to whom anatomy owes such beautiful and important discoveries, first pointed out the dependence which exists between these two systems.

Having acquired a knowledge of the organization of the principal articulated animals, we were enabled to rise to general considerations respecting the classification of the beings composing this family. We particularly devoted our attention to the arachnides, the animals of that kind the least circumscribed. We have endeavoured to demonstrate that in a rigorous method of classification we ought not to unite animals which have a heart and circumscribed respiratory organs with those that have no heart and ramified respiratory organs. Accordingly we have not adopted the class of

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arachnides, such at least as it has been conceived by modern naturalists. We have, on the contrary, restrained it to the cloportides, scorpionides, and arachnides. They have all an elongated heart, with two orders of blood-vessels, and circumscribed respiratory organs, composed of pyramidal leaves, exhibiting this remarkable particularity, that they receive the air directly by stigmatiform openings. But it is with hesitation that we have placed the cloportes among the arachnides. What we kı ow of their organization pointed out that union so strongly that we have not ventured to place them among the crustaceous animals, with which, however, they have various relations. We have put the cloportes at the head of the arachnides, and immediately after the crustacea, in order to point out the relation which exists between the first and last orders of these two classes. We see that the cloportides constitute the transition from the one to the other. By adopting this mode of classification we have been obliged to begin the study of insects with those species which have the greatest affinity with the spiders, the last order of the arachnides. In this respect there was no difficulty in making a choice; the phalangites placed themselves, as it were, of their own accord at the head of the insects. However, for a contrary reason, we have been obliged to separate the *jules* and scolopendra, which in some respects have an analogy with the cloportides. In seeking for the arrangement which would the least interrupt the natural relations which the articulated animals have to each other, we were convinced that the characters drawn from the organs most essential to life could not unite them in such natural groupes as they do the animals with vertebræ. This is one of the great difficulties in their classification—a difficulty probably proceeding from the smaller influence which the organs of circulation and respiration have in proportion as the organization becomes more simple.

We have thought it equally necessary to separate those insects which undergo transformations from those which, like the arachnides, do not undergo any. We have divided the whole into two great divisions, founded solely upon this transformation, which in one division takes place by degrees, while in the other it operates, as it were, instantaneously. In consequence of this great division we have been obliged to form an additional order among the insects. This order, which we have called *syphon-aptera*, connects very well the insects that undergo a transformation with those that do not. Besides, it commences in a very natural manner the order of insects that are suckers. These lead us to the other orders of suckers from which we pass to the bruisers, which, being more complete insects (if the term may be allowed), ought to be at the greatest distance from the arachnides; as their organization is the most different from that of the arachnides that we observe in any animals of this class.

We cannot deny that in a task so difficult as that of which we have given a sketch we have left many gaps to fill up. But we have

at least examined ourselves all the details of organization which we have made known in our memoir. In this point of view it will always have a real value. We must acknowledge that in the anatomy of animals so small, and so difficult to dissect, it is almost impossible to avoid falling into some errors. We ought to consider it as fortunate when these are confined to details which have not a great influence on the organization. It is in this point of view that it is of importance to science that different observers study the same subject. It is only in that way that we can be sure of having made good observations. We cannot terminate what we have to say respecting our researches without informing those who may be devoted to the same kind of labour that the best means of avoiding errors is to dissect in water, as Cuvier has advised. It is proper likewise to employ wires of copper and zinc to discover the nerves, according to the method recommended by Humboldt.

Insects have a longitudinal vessel along the back, which extends from the upper part of their body to the other extremity, and to which almost all anatomists have given the name of heart, in consequence of the regular contractions and dilatations which it exhibits. Lyonnet and Cuvier alone have started doubts respecting the reality of the functions ascribed to this organ. The illustrious author of the Comparative Anatomy is likewise the first who endeavoured to prove that this heart has no sort of ramification. Lyonnet had already remarked that no smaller vessel issues from the dorsal vessel. But as he observed this fact only in the caterpillar of the willow he did not venture to draw a general conclusion from an observation which might be only particular. Yet, notwithstanding the numerous proofs of all kinds accumulated by M. Cuvier in his fine memoir on the nourishment of insects * against the existence of a true heart in this order of animals, doubts still continue respecting this delicate question among a considerable number of anatomists. To endeavour to destroy these we have entered into some researches respecting this interesting point.

We have turned our chief attention to the following questions:—

1. It appeared to us essential to study the dorsal vessel in the different orders of insects, in order to know whether it ever presented ramifications, which might be expected in it, because Swammerdam and Comparetti distinctly say that they observed them. Comparetti has even described them somewhat in detail.

2. If this first means proved insufficient, it was necessary to ascertain whether they might not become visible by injections either of mercury or coloured liquids, or astringent liquors, or such as by their chemical properties are capable of acting upon the *humor* contained in the dorsal vessel. Thus tannin, by coagulating that humor, appeared capable of throwing some light on the uses of this vessel, and of showing its ramifications, if any such existed.

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* Memoire sur la Maniere dont se fait la Nutrition dans les Insectes, inserted in the Memoirs of the Natural History Society of Paris, t. i. p. 34.

3. It was no less essential to ascertain if the contractions and dilatations of the dorsal vessel were owing to the action o ithe fluid contained in it.   On this supposition it would be necessary to show a kind of circulation of this fluid : while according to the other hypothesis it became necessary to show that these contractions and dilatations were not produced by the humor contained in the dorsal vessel, but might be stopped without altering that vessel.

4. The organs of circulation appearing always to bear a relation to the organs of respiration, it was necessary to examine if the dorsal vessel was formed in the same manner in the species which respire air directly, and in those which respire it contained in water, or which decompose that liquid.

5. All these researches ought necessarily to enable us to determine the influence of the organs of respiration on those of circulation.   If the influence of the first were demonstrated to act completely on the second, we ought to observe that, in proportion as the organs of respiration concentrate, or to speak more correctly, cease to ramify, the organs of circulation will exhibit ramifications, since the air going no longer in search of the blood it is necessary that the blood should go in search of the air.

Such was the object of my researches. Before giving an account of them I think I ought to state the opinions of the different anatomists respecting the dorsal vessel.

Malpighi * has described the dorsal vessel in his excellent anatomy of the silk-worm.   He considered it as a long canal, which in its length swells out or contracts, forming several small oval tubes, which may be regarded as so many small hearts.   But here Malpighi was deceived by the successive contractions of the dorsal vessel.   What proves this are the remarks which he makes himself.   The movements, says he, of those parts of the dorsal vessel, which we may consider as so many hearts, are very irregular, and the liquid contained in it has often a retrograde motion.   This irregular motion cannot agree, as Cuvier has remarked, with a principal organ of circulation.   This single proof seems sufficient to prevent us from considering the dorsal vessel as a heart. Malpighi adds that he never perceived the least ramification in the dorsal vessel; and in fact the figures which he gives of it (tom. ii. tab. iii. fig. 4,) do not exhibit any.   But how can we conceive a circulation without vessels, and how can we give the name of heart to a vessel which performs none of the functions of that kind of circulation ?

Malpighi inquires next whether arteries proceed from this heart, and whether they convey the blood to the whole body.   As to the branches which come from the dilatations of the dorsal vessel, and which he calls *auricles,* he recognized them for tracheæ.   That they are so is evident, as their extremities that touch the dorsal vessel

are the smallest, while the contrary would be the case if they pro-
ceeded from the vessel.

Swammerdam has described and figured the dorsal vessel of
insects with more accuracy than Malpighi; but, like him, he
always considered it as a heart, or at least as an organ which per-
formed the functions of one. In giving a description of the insects
of his cabinet this excellent observer says expressly, " I preserve
eggs of the grasshopper, which are oblong, and between which
appear blood-vessels both veinous and arterial."* But as Swam-
merdam takes no further notice of this observation, except in
speaking of the silk-worm,† it is probable that he was misled by
the brownish aspect which the tracheæ assumed in drying. A very
simple cause may have equally contributed to this illusion. The
inferior hepatic vessels are extremely elongated in the grasshoppers,
and often run along the ovarium. Hence when great attention is
not paid to them they may be easily mistaken for blood-vessels.
Swammerdam describes the dorsal vessel in his anatomical re-
searches as a hollow vessel, exhibiting at intervals dilatations and
contractions.‡ He is far, however, from considering these con-
tractions as so many hearts, and from believing that the liquid
which they contain retrogrades. He even opposes the opinion of
Malpighi, acknowledging, at the same time, that he does not
know how the bronchiæ communicate with the branches of the
tracheæ, and these with the heart. Speaking of the larva of the
geotrupa nasicorna, Swammerdam observes, that along the whole
heart there exist moveable fibres, similar to the rings of the body.
These fibres, according to him, penetrate into the substance of the
heart itself, forming as many cords which dilate and contract it. It
is these moving fibres that make the dorsal vessel appear as if it
were composed of a series of contractions and dilatations, which
have deceived almost all observers. On removing them we see
very well, as Cuvier has observed, that this vessel is a simple tube,
equal through its whole length, and only smaller towards its extre-
mities.

De Geer has taken so little notice of the dorsal vessel that it is
difficult to know if he was sufficiently sensible of its importance.
His ideas can be of no weight in the question which occupies us;
accordingly we do not think it necessary to pay more attention to
them.

As to the opinion of Lyonnet, it is very different from that of
those observers whom we have already mentioned. This skilful
anatomist does not think that the dorsal vessel of insects is entitled
to the name of heart, as he could never discover either veins or
arteries attached to it, with whatever care he made his injections

* Biblia Naturæ, t. i. p. 214.
† Collection Academique, t. v. p. 264.
‡ Biblia Naturæ, t. ii. tab. xv. fig. 4; t. i. p. 252; tab. xxviii. fig. 8; t. i.
p. 311; t. ii. p. 577; tab. xxxiv. fig. 6, *a a*; t. i., p. 664, tab. xl. fig. 4.

with ink and coloured liquids. He concludes from this absence of blood-vessels, and from the communication which exists by means of fibrillæ between all the internal parts of insects, and from the fatty bodies spread every where, that these bodies draw their nourishment from the fat by means of the fibrillæ. Hence other uses of the dorsal vessel must be sought for, and the one pointed out by Lyonnet does not appear to be the least essential, as the facts which we shall state in this memoir will show.

Comparetti, to whom we owe a great number of anatomical observations, and who had the glory of labouring with Scarpa in a very difficult undertaking, is the only person who has admitted without any restriction a double vascular system in insects properly so called. This double system extends, according to him, to all the membranes and all the viscera. It is particularly related to the muscular system.* By this admission of a double system, Comparetti was forced to conclude that the liquid contained in the dorsal vessel has a circulation, and that it flows upwards, as in the common earth-worm.† He thinks likewise that the contractions and dilatations of the dorsal vessel are not equal, and that they are more or less considerable, according as they are examined in different parts.

Passing then to the examination of the vascular vessels, he describes them in the fly, observing that the lateral trunks which have movements of contraction terminate in two white and oval bodies. Two vascular trunks give, according to him, large branches, diverging on the internal surface of the ring as on the heart and œsophagus. He says likewise that he perceived radiated filaments on the surface of the two muscles at the side of the ring, and there to have reckoned 150 pulsations in a minute. As to the movement of contraction and dilatation, it appeared to him to commence at the base, and to continue several hours after he had made a lateral section.‡ In the bee-formed fly, which he calls *mosca apiformis*, he says he observed the dorsal vessel presenting lateral and vibrating ramifications. These ramifications communicate with others, which extend in the abdomen, and vibrate like the heart. Some of them exhibit a pretty frequent pulsation, others very striking undulations. I have often observed in a great number of insects, especially in the scolopendras, very strong contractions of the muscular fibres, especially in those which go to the claws. These contractions extend transversely from one side of the muscles to another, and are so distinct that we might believe them to be produced by some ramifications of the dorsal vessel ; but as this vessel has no ramifications, these contractions must depend upon the muscles themselves. Hence Comparetti might have been easily deceived by these contractions, and might have believed them produced by branches of the dorsal vessel, especially as, in some species, the tracheæ have a violet colour, or a dark red with a shade of blue. Comparetti is not satisfied with describing the pulsations of the ramifications of the

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* Dinamica Animale, Part I. p. 236.   † Ibid. p. 211.   ‡ Ibid. p. 216.

dorsal vessel in one species, but says that he observed them likewise in the viviparous fly figured by Reaumur, * and also in the *syrphus inanis*.† He goes still farther in speaking of the bee, affirming that he observed in it two canals or cylindrical vessels issuing from the extremity of the dorsal vessel, one of which goes to the upper part of the body, and the other to the lower. These vessels furnish others, all of which have vibrations more or less distinct.‡ In continuing his researches he speaks always of the vibrations of the branches furnished by the dorsal vessel. This he does in speaking of butterflies, tenebrions, and the house cricket.§ Finally, when he describes the dorsal vessel of the blattas, a vessel which he always calls *heart*, he says that the blood contained in it passes into all the internal parts, and that the vascular system extends to the finest branches both of the external part, and of the internal of the abdomen, thorax, head, and limbs.‖

This short statement of the observations of Comparetti proves not only that this great anatomist entertained no doubts that the dorsal vessel had ramifications, but that he thought he found in the insects, as well as molusca, a double vascular system. It is not easy to conceive how Comparetti was deceived in his dissections, which, though delicate, are not difficult, in consequence of the small number of vessels which exist in insects. To discover the cause of his error, we have repeated most of his dissections with the greatest care upon the very species which Comparetti points out himself. But all our researches have tended to convince us that there exist no ramifications of the dorsal vessel of insects, nor vascular system in that order of animals. This is not the case in the scorpions and spiders. Accordingly these animals, which have a vascular system, present as a necessary consequence circumscribed respiratory organs, in place of the tracheæ which we find in all the true insects. But distrusting our own observations, we got them repeated by a young anatomist, M. Pilar, with whose accuracy we were well acquainted. Notwithstanding all the patient industry which he bestowed on this subject, he was unable to discover the smallest ramification from the dorsal vessel of true insects. This fact being fully ascertained, we suspect that what were taken by Comparetti for vascular vessels are nothing else than the inferior hepatic vessels. These vessels are very long, free, and almost capillary. As they have usually a dark colour, they might be easily taken for veins or arteries. This is the more probable because they have often a vermiform motion. This is the movement which Comparetti took for vibrations.

Since the time of Comparetti, M. Cuvier has established by a great number of negative proofs, as well as by reasoning, that the dorsal vessel in insects has no ramifications, and that the organization of these animals rendered such ramifications useless. He has

* Tome iv. p. 11.   Mem. x. p. 161.
† Dinamica Animale, Part. 1. p. 217, 219.
‡ Ibid. p. 221.   § Ibid. p. 223, 228, 227.   ‖ Ibid. p. 230.

recently considered the dorsal vessel as a vestige of a heart, or as a viscus which does not perform any of the functions of that organ. Before an organ ceases to exist it no longer performs the functions which it performed in those animals where its presence is absolutely necessary. He mentions, as an instance of this cessation of functions before the organ disappears, the hands, which still exist in a great number of species, though they are no longer of any utility as hands.

From the statement which we have now made it is evident that the non-existence of ramifications of the dorsal vessel has been admitted by the greater number of anatomists. But as doubts still exist on the subject, because some observers describe these ramifications with details which appear the result of the most attentive examination, we have thought it worth while to undertake the researches, of which we shall now give an account.

(*To be continued.*)

Article VI.

Experiments to determine the Quantity of Oxygen in the Oxides of Cerium.   By W. Hisinger.[*]

As all the attempts hitherto made to reduce the oxide of cerium to the metallic state have failed of success, chemists cannot determine the quantity of oxygen which the oxides of that metal contain by direct solution of it in acids. I have therefore had recourse to the method pointed out by Berzelius in his dissertations on the chemical proportions in which bodies unite; namely, to determine the proportion of oxygen in the bases by the quantity of that principle contained in the weight of acid with which a given portion of these bases unites.

Pure muriatic acid was saturated by long boiling with oxide of cerium carefully purified. The greatest part of the oxide was dissolved in the acid. The solution was boiled down to dryness, in order to decompose all the insoluble salt which it contained. It was then dissolved in water, and by that means separated from the superabundant oxide. Nitrate of silver was dropped into the solution as long as any precipitate fell. The precipitate was separated by means of a filter carefully weighed. It was washed, and melted by the heat of a lamp, in a small glass capsule. Its weight, reckoning 0·018 gramme (0·278 grain), which remained on the filter, amounted to 1·819 gramme (28·09 grain).

The solution was diluted with water, and freed from silver by means of muriatic acid. The oxide of cerium was then precipitated

by carbonate of ammonia. This precipitate, being heated to redness in a close vessel to expel all the alkali, weighed 0·684 gramme (10·01 grain).

Now as 100 parts of fused muriate of silver contain 19·05 parts of muriatic acid, and as 684 parts of oxide of cerium combine with a quantity of muriatic acid which forms 1819 parts of muriate of silver, it follows that 684 parts of oxide of cerium combine with 346·5 parts of muriatic acid. Therefore muriate of cerium is composed of

Muriatic acid .... 33·624 100
Oxide of cerium .... 66·376 ......197·5
                        —————   ————
                        100·000 ......297·5

But as 100 parts of muriatic acid are saturated by a quantity of base containing 29·45 parts of oxygen, it follows that 197·5 parts of oxide of cerium must contain 29·45 of oxygen. Hence 100 parts of this oxide consist of

Metal .... 85·088 100
Oxygen .... 14·912 ...... 17·41
                     ————   ————
                     100·000   117·41

Farther, as by the laws established in the dissertation of Berzelius above referred to, an oxidule, in order to be converted into an oxide, combines either with half the quantity of oxygen which it already contains, or with an equal quantity, it is evident that the peroxide of cerium must be composed of 100 parts metal united either with 26·115 or with 34·82 parts of oxygen.

The analyses of the carbonates appear to prove that the peroxide of cerium contains 1½ times as much oxygen united to the same quantity of metal as the protoxide.

I have (in the Afhandl. i Fysik Kemi och Mineralogie, 3 D. sid. 287,) given an analysis of the oxides of cerium united with carbonic acid; and I found that the procarbonate, which, when decomposed in a retort, gives very evident indications that it contains water, leaves 57·9 parts of pure protoxide. The experiments of Berzelius appear to show that carbonic acid neutralizes a quantity of base containing half as much oxygen as exists in the acid, and that the chemically combined water, in like manner, contains a quantity of oxygen which is a multiple of that contained in the acid. If we reckon the constitution of the procarbonate of cerium according to these principles, it is obvious that the 42·1 parts driven off by the heat must consist of 23 parts of carbonic acid and 19·1 parts of water; 57·9 parts of the protoxide contain 8·63 parts of oxygen; and 23 parts of carbonic acid (reckoning the oxygen at 73 per cent.) contain 16·19 oxygen, while 19·1 parts of water contain 16·95 of oxygen. These two quantities (abstracting the small anomaly) are each double the quantity of oxygen in the protoxide.

Now it is obvious that the anomaly cannot occasion any difficulty, as it is within the limits of the errors of the analysis itself.

I found the percarbonate composed of 63·83 parts of oxide and 36·17 parts of carbonic acid. This quantity of acid contains 26·50 of oxygen, one half of which = 13·25 must exist in the 63·83 parts of oxide. Hence that portion of oxide contains 50·58 parts of metal; but 50·58 : 13·25 :: 100 : 26·195. Now 17·41, or the quantity of oxygen combined with 100, metal in the protoxide multiplied by $1\frac{1}{2}$ = 26·115.

We cannot certainly expect a nearer coincidence than this between calculation and experiment. Of consequence the peroxide of cerium must be composed of

Metal	79·29	......100·000
Oxygen	20·71	...... 26·115
	100·00	126·115

Article VII.

*Experiments to determine the Proportion in which Bismuth unites with Sulphur and Oxygen. By P. Lagerhjelm.**

1. *With Sulphur.*

The bismuth of commerce was boiled in a solution of aqua regia. The solution, being saturated with ammonia, and mixed with some prussiate of potash, gave a Berlin blue precipitate, indicating the presence of iron. The bismuth was reduced to a state of purity by the following method :—

The aqueous solution was precipitated by water. The precipitate was edulcorated, and dried by the heat of a stove. Seven parts of this dry mass were mixed with one part of charcoal powder and five parts of black flux. This mixture was put into a large crucible, and exposed to a red heat for a quarter of an hour. There were obtained $\frac{14}{3}$ of pure metal.

The sulphur which was employed in the following experiments was partly roll sulphur purified by distillation, partly native sulphur. It was heated before each experiment, sometimes nearly to the melting point, and sometimes it was rendered quite liquid, for its more commodious application to the metal.

Into a dry glass vessel, weighing 2·979 grammes, there were put 10,000 grammes of metal in the state of a fine powder, and abundance of sulphur. The glass vessel was heated over a spirit lamp

till the mixture detonated. The heat became in consequence so high that the mass immediately became red-hot. The glass vessel was now exposed to a red heat, in order to drive off any excess of sulphur that might be present.

When cold it weighed ....,.....15·2266
But the weight of the glass was...... 2·9790
————
12·2476

Hence ten grammes of metal combined with 2·2476 of sulphur. To prevent the oxidizement of the bismuth in this operation, the neck of the glass capsule was drawn out long and slender. The colour of the sulphur which sublimed showed me that it carried no bismuth along with it. I was not able to make experiments that agreed with each other in such capsules. I therefore made choice of small retorts with long and slender beaks, and I put the sulphur into the bottom of the vessel. Many of my experiments failed, because the retorts were not able to endure the sudden elevation of temperature occasioned by the spontaneous combustion of the mixture which they contained.

Ten grammes of metal were put into a small retort, and mixed with a sufficient quantity of sulphur.

Grammes.

The retort, after being exposed to a red heat, weighed ....31·8075
Subtracting the weight of the glass = ...,............19·5800
————
Remains for the weight of the sulphuret of bismuth formed ..12·2275

The difference between this experiment and the preceding is considerable; but I found that a weight of 50 grammes, which had been employed in weighing the retort, was 0·0245 too light. This deficiency must be added to our result, which will bring it to 12·2520.

Subsequent experiments, made in the same way, gave the following results :—

12·2065
12·2230
12·2465

These results vary from each other in the second decimal place. We must defer determining which of them is nearest the truth till we have ascertained the quantity of oxygen with which bismuth unites. Small differences in our experiments are unavoidable; but we have it in our power, notwithstanding, to determine on which side the truth lies.

2. *With Oxygen.*

Into a dry and clean glass vessel, which weighed 37·1775, there were put 8·5045 parts of metal, which was boiled with nitric acid.

The whole, being boiled to dryness, that the metal might be oxidized, the glass vessel was heated to redness, and weighed 46·6500. Now the weight of the

Glass was .... = 37·1775
Metal .... = 8·5045
Oxygen .... = 0·9680
                                ―――――――
                                  46·6500

But 8·5045 : 0·9680 :: 10 : 1·1382. Therefore in this experiment 100 parts of metal combined with 11·382 parts of oxygen. The principal part of the mass had a high colour (like that of gum-gut) ; but some red streaks were perceptible in it ; and when viewed in a particular position, the whole mass appeared red, like the rust of iron. This might proceed from a portion of iron contained in the acid employed, and which was rendered visible by the great quantity of acid boiled over the metal. The experiment being repeated, 100 parts of metal united with 11·275 parts of oxygen. The whole mass, as in the first experiment, when held in a particular position, had a tile-red colour. The acid employed was pure, and therefore could not communicate any iron to the oxide. The mass was exposed to a stronger red heat than in the former experiment. This circumstance, together with the purity of the acid employed, may account for the smaller per centage of oxygen which was found to unite with the metal.

Experiments made in this way are not liable to any loss. As the second seems most worthy of confidence, we may reckon the quantity of oxygen which unites with 100 parts of bismuth at 11·275.

3. *With Sulphur and Oxygen.*

Into a glass capsule, weighing 31·882, were put 10 parts of metallic bismuth, which were boiled with sulphuric acid to a grey mass. This was sprinkled with nitric acid, in order to convert the whole into sulphate of bismuth. The boiling was continued till the whole mass became white, and then the capsule was exposed to a strong heat, in such a manner that the sulphuric acid driven off could not condense in the mouth of the vessel. The mass, when cold, inclined a little to a grey colour, and weighed (including the vessel) 48·337. Thus we see that 10 parts of metal had increased in weight 6·455. Of this increase, 1·1275 was oxygen, and 5·327 sulphuric acid. Now this quantity of sulphuric acid contains 3·1946 of oxygen. But this number is not a multiple of the oxygen in the metal by a whole number. This circumstance, combined with the colour, led me to suspect that the metal had not been all oxidized. On that account nitric acid was added, which occasioned the emission of red fumes. The whole, being boiled for some time, evaporated to dryness; and exposed to a gentle red heat,

assumed a snow-white colour, with the exception of a flock or two of yellow.* The weight was now 48·664, constituting an addition amounting to 6·782. This consisted of

Oxygen in the oxide .... 1·128
Sulphuric acid .... 5·654

Now this portion of acid contains 3·3907 of oxygen. For 100 : 59·97 :: 5·654 : 3·3907. And $\frac{3\cdot3907}{3} = 1\cdot1302$, a number which surpasses the quantity of oxygen contained in the oxide only by 0·002. That the salt was pure sulphate of bismuth was evident from its snow-white colour, from the separation of sulphuric acid during the exposure to a red heat, and from the homogeneity of the mass both in colour and texture. The difference between the temperature which is sufficient to drive off the excess of acid, and which is requisite to decompose the salt, is so great that there is no difficulty in making this experiment with accuracy. It is obviously necessary to expose the neck of the glass to the same temperature as the vessel itself, in order to prevent the sulphuric acid from condensing in it.

———

The laws according to which bodies unite and separate from each other are invariable. Yet it is impossible to expect that the results of various experiments should perfectly agree with each other ; even vision itself is liable to be misinterpreted. The experiment which seems to come nearest the truth is the saturation of the metal with oxygen. Therefore I assume that 100 parts of metal combine with 11·28 of oxygen. From this it follows that 100 parts of metal become 167·71 parts of sulphate of bismuth. This salt is composed as follows :—

Base .... $\begin{cases} \text{Bismuth .... } 59\cdot627 \\ \text{Oxygen .... } 6\cdot726 \end{cases}$
———— 66·353

Acid .... $\begin{cases} \text{Sulphur .... } 13\cdot469 \\ \text{Oxygen .... } 20\cdot178 \end{cases}$
———— 33·647

100·000

In this salt there exist 10 parts of metal and 2·259 parts of sulphur. In the preceding experiments the greatest quantity of sulphur combined with 10 bismuth was 2·252. This of course comes nearest the truth.

———

* Probably pure oxide. The want of oxygen in the first operation had occasioned the formation of some sulphite ; but the additional nitric acid converted these portions into sulphate. Perhaps a little oxide might be developed by the red heat.

Results.

Oxide of bismuth composed of.........	{ Metal ...... 89·863	.... 100·00
	{ Oxygen .... 10·137	.... 11·28
	100·000	

Sulphate of bismuth ..	{ Base .... 66·353	.... 100·00
	{ Acid ...... 33·647	.... 50·71
	100·000	

Sulphuret of bismuth..	{ Metal ...... 81·619	.... 100·00
	{ Sulphur .... 18·381	.... 22·52
	100·000	

Article VIII.

On a New Solvent for all Sorts of Urinary Concretions. By Mr. C. R. Goring.

Chemistry has been long looked up to, and as yet in vain, for a remedy for that excruciating disease, a stone in the bladder. It has been proposed to ascertain the composition of the calculus, and then inject its liquid solvent; but this method has failed of success in the application, or rather cannot be applied at all, for a thousand reasons, which must be evident at first sight. It appears to me, however, that there is an agent fully capable of destroying *all sorts* of calculi, and at the same time applicable without risk to the patient. It is that wonderful energy—galvanic electricity. This power, it would appear, is capable of being accumulated to any intensity by keeping up a proper proportion between the number and size of the plates composing the battery, and its enormous power of decomposition is well known; it is much more than sufficient to subvert any union subsisting between the constituents of a concretion which have all been ascertained to consist either of acid, anti-acid, or generally both, with some animal matter. Now it matters not what particular acids, &c. enter into the constitution of a concretion, as galvanism is equally a match for every one, and overpowers the strongest attraction equally with the weakest; it will infallibly drag the anti-acid to the negative, and the acid to the positive wire. The way then I think galvanism might be applied is this: Let a puncture be made in the bladder with a common trochar as usual (this I believe is an operation attended with no risk, and no great inconvenience). Introducing any thing into the bladder through the urethra is a source of very great irritation to the patient, and had better be avoided for this lesser evil. Let the

canula be left in the aperture ; then procure a couple of platina wires inclosed in a glass tube, but carefully insulated from each other, and projecting at the end of the tube ; the tube must slide easily in the canula, and be made of a convenient length ; the wires of course are to be connected with the battery ; then let the patient himself (lying on his back,) take the tube and guide it through the canula to the stone at his leisure ; he will probably be able to do so better than anybody else. The operation may be expected to proceed thus : When the ends of the wires projecting from the tube come in contact with the stone they will begin to attack it ; they will at the same time decompose the urine and its salts, &c. ; the watery part will be decomposed, and resolved into hydrogen and oxygen, and escape through the interval between the tube and the canula. I may mention what a fortunate thing it is that galvanism does not produce a spark at any sensible distance between the wires ; if it did, the hazard, indeed certainty, of exploding the hydrogen and oxygen, must have precluded the use of it, as the wires must necessarily be near each other, and when withdrawn from the canula have come in contact with the gaseous matter escaping from the bladder. It will not be amiss, now it has become so easy a matter, to pour some tepid water from time to time into the bladder, to afford a solvent medium for the unnatural quantity of salts found in the urine of patients with calculous complaints, and prevent farther deposition, and consequent increase in the size of the concretion, which might go a great way in undoing all that was done by the galvanism. Indeed, the water may in some cases take somewhat of a solvent part. Uric acid, of which calculi often consist, is slightly soluble in water, and so are some urates. The application of the galvanism must be continued till the stone is altogether dissolved, or become small enough to be evacuated through the urethra. It must necessarily be a work of some time ; but if the calculus can be destroyed even in the period which it took to be formed, the patient could hardly complain ; he may soon be taught to manage every thing for himself at his leisure. To conclude, galvanic electricity has performed many wonders already ; and I cannot help thinking that if there is a remedy beside the knife for a stone in the bladder, it will be found in its powers of decomposition. However, experiment alone must satisfy us.

ARTICLE IX.

Observations on a Luminous Arch which appeared in the Sky on the Night of Sunday, September 11, 1814. By J. B. Longmire.

ON the night of Sunday, the 11th of September last, a very beautiful meteoric object presented itself to the view of the people

In that part of the west of England which lies opposite the Irish Sea, to those in the west part of the south of Scotland, and to those in the middle part of the west of Ireland. During the time of its appearance the air was apparently still, and so clear that more stars were visible than can be generally observed in star-light nights.

This phenomenon was a part of either a body of dense light, or a mass of luminous matter, in the shape of an arch ; whose curvature was considerably greater than that of the earth, but whose middle part was flattened into a horizontal line, for a distance which is equal to half the length of the visible part ; and whose direction, when I first saw it, was N. 80 E. and S. 80 W. The colour of this arch, which was greyish white, resembled very much that of the white parts of the clouds when the sun shines upon them. Its lustre was weak. Its light, when I saw it, was not so intense as to illuminate the air, except in its immediate vicinity ; where the light, as I am informed, extended to the ground, and probably as far in every other direction : and it was so transparent that through the western part stars of the first and second magnitudes could be seen ; through its middle part, those so small as the third rate were dimly seen ; and through its eastern part, those to the fourth rate were just visible ; while the stars contiguous to both sides of it, to the fourth magnitude, were *very distinctly* seen.

The figure of the arch was in general well defined : but in a few places near the lines which joined together its sides it appeared to be softened by being mixed with the air, like a light-coloured cloud just before it gives out rain. Such softened parts appeared for a small time in certain situations, then disappeared ; and afterwards others did the same in other situations. These appearances took place occasionally when I saw the arch ; and they were owing, I suppose, to the gradual separation of small portions from the great body of luminous matter.

After the surprise had subsided which was excited by this object's appearance, I made every observation in my power to ascertain its dimensions and distance from the earth, and I soon acquired every necessary particular for these purposes, except its distance from me. To obtain this distance I had not then the means ; but a week after its appearance I observed a paragraph in the Lancaster newspaper, in which it was said to have " extended from the west *across the meridian* to the N. E. by E. soon after eight o'clock." The time at which my observations commenced must have been so near that time that I proceeded in my calculations immediately after reading this paragraph.

The straight line between me and the eastern extremity of the view was S. 60 E., and that between me and the western extremity S. 77 W. ; so that the visible part of the arch subtended an angle of 107°. Now as I was 27 miles N. 11 W. from Lancaster, by conceiving a line whose direction is S. 60 E. and N. 60 W., or the direction of the arch at the time of observation, to be drawn across

the meridian of this town, till it meets the lines between me and the extremities of the arch's visible part, then it will intersect the eastern line near the town of Thorns, in the West Riding of Yorkshire; and the western line at a point about 30 miles N. E. by E. of St. Hilary Point, in the Isle of Anglesea; having an intermediate distance for the visible part of the arch of about 74 miles. As the arch at the western extremity was reduced nearly to a point, it could not extend much farther in this direction; but as its dimensions increased toward the west, the distance to which it might extend beyond my view in this direction is not attainable.

The height from a horizontal line drawn from where I stood to the middle part of the arch subtended an angle of 17°, which makes its altitude $7\frac{1}{2}$ miles above that line. As I cannot be certain that my observations were made exactly at the same moment as those at Lancaster, I will not maintain the altitude of the arch to be *precisely* what I have made it, but it probably approximates so near to the truth that the extremes of possibility will not make it more than nine nor less than six miles.

The depth of the arch at the western extremity subtended an angle of 1° 15′, at its middle 52′, and at the eastern part 15′: from which observations it appears that its depth at the first place was 1504 yards, at the second place 1041 yards, and at the third place 299 yards.

The breadth of the arch at the western end subtended an angle of only 8′, and at the middle an angle of 5′, but at the eastern end the breadth was scarcely visible; so that the breadth of the arch would be nearly equal to half its depth.

Such was the arch when I first subjected it to observation. But prior to that time, as I was informed by two or three persons, its length increased from west to east, as if it had been slowly projected in that direction. It was then considerably to the north of Kendal.

It appears from accounts published in the newspapers that it was seen at Dublin and Newry, in Ireland; at the latter place, at 7^h 30′ *p. m.*; in Scotland, at Glasgow, Dumfries, and Annan, about the same time; in England, at Whitehaven, at 7^h 45′; at Carlisle, Kendal, and Lancaster, at 8^h 10′ *p. m.*

The luminous arch moved to the southward. This motion, I think, was caused by the current of the air being in this direction. The air, indeed, had no sensible motion where I was; but we can often see the clouds to move when we do not feel the wind that moves them. This motion was not equal in every part of the arch; for when I first observed it its direction was N. 80 E. and S. 80 W.; when it had reached Lancaster it was N. 60 E. and S. 60 W.; and when it was opposite Derbyshire its direction at one time was N. 75·45 E. and S. 75·45 W. During these changes of the arch's direction its western part must first have been moved faster than its eastern part for a certain distance; then its eastern faster than its western part for the remainder of the distance between a small distance on this side of Lancaster till when it was opposite Derby-

shire. It had occasionally a slow and small undulating motion perpendicularly. Such irregularities as these in the motion of the arch are not at variance with those which are known to take place in that of every current of air whose breadth is considerably less than the length of the arch in question.

At 8^h 20′ the arch disappeared at the eastern end, and there its matter was diffused through the atmosphere; at 8^h 25′ it disappeared at the western end of the view, under similar circumstances; and its disappearance continued gradually in both directions toward its middle; but from the eastern part of the arch it proceeded so much faster than from the western part, that the arch became extinct at a point which was situated from the latter place a distance only equal to one third of the whole distance of the visible part. After the arch's disappearance there remained throughout the whole of its original length a comparatively subtile light, or luminous matter, of a greyish-white colour, that had the glow of the faintly-coloured electrical fluid. Through this luminous body nearly all the stars could be seen that were visible before it was interposed between them and the observer. This light was not diffused through the atmosphere in every direction, but was arranged in the shape of a stratum, whose under surface was nearly parallel to the curvature of the earth. The luminous body in this state, and when it composed the arch, did not possess the corruscant appearance of the Aurora Borealis.

After the arch disappeared, several large clouds of faintly luminous bodies, each nearly similar to that just described in its attenuated state, occasionally passed over us to the south. Some of these clouds were so low, that at Lancaster they appeared only a small distance above the houses' tops. The Aurora Borealis also was active from eight o'clock in the evening till three in the morning, in the northern part of the hemisphere.

A luminous arch, which was nearly similar to that just described, was observed at Kendal and Dublin on the 17th of last April. Many persons in this county have seen arches several years since, which strongly resembled the arch so lately seen.

As the arch disappeared because its matter assumed much larger and more irregular dimensions, and not that it had got beyond the distance of vision, I had considered its existence, as an arch, at an end; and therefore I was much surprised on hearing that a similar arch was seen at Liverpool between eight and nine; at Beardsall, in Derbyshire, about nine; and at Warrington, at ten minutes before ten o'clock; on the same Sunday night. Now as from all accounts this must have been the same arch, or at least an arch composed of the same matter as that which I saw; either the luminous matter which was diffused through the atmosphere on the arch's disappearance was only a small part of the arch, which had been detached from it by some cause, and which kept the arch then immediately behind it from my view; or all the arch's matter had been dispersed by some temporary-acting force, and when this

force ceased to act the luminous matter so dispersed assumed a shape and dimensions which were nearly similar to those of the former arch.

This grand meteoric object differs very much from common meteors; from solar and lunar bows; and from the Aurora Borealis, or northern lights; and all other known electrical phenomena. It is sufficiently distinguishable from common meteors by its shape being so different, its size so much larger, and its motion so much slower than theirs. It differs from the bows by having length, depth, and breadth, and thereby being *a body*; whereas they have only length and depth, and may be considered as only *surfaces.* They are caused by the division of light into parts, and are therefore of various colours; but it was white only. Lastly, light emanated from it; but it never does so from them. It differs from the Aurora Borealis in wanting their coruscant property, and in having a much paler light than theirs. And if it were the electrical fluid, its long continuance in the atmosphere, *in a state of rest, and yet luminous,* makes it such a modification of this fluid as we are not much acquainted with.

Troutbeck, near Kendal, Sept. 30, 1814.

ARTICLE X.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32' 40·3" North. Longitude West in Time 6″$\frac{8\,2}{1\,0\,0}$.

Sept. 27, Emersion of 3 ♃ ♒ 8ʰ 22' 18" Mean Time at Hackney Wick.
Oct. 1, μ Ceti $\begin{cases} \text{Immersion} & 10 \quad 29 \quad 22 \\ \text{Emersion..} & 11 \quad 30 \quad 26 \end{cases}$ Mean Time at Hackney Wick.

Magnetical Observations.

1814.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
Sept. 18	8ʰ	25'	24°	11'	55"	1ʰ	45'	24°	23'	06"	6ʰ	15'	24°	15'	14"
Ditto 19	8	40	24	15	37	—	—	—	—	—	6	15	24	14	02
Ditto 20	8	30	24	14	06	1	45	24	21	55	6	15	24	15	28
Ditto 21	8	30	24	15	27	1	40	24	22	56	6	15	24	15	50
Ditto 22	8	30	24	14	45	1	35	24	22	03	6	05	24	16	40
Ditto 23	8	25	24	13	30	1	35	24	20	48	6	00	24	14	44
Ditto 24	8	35	24	12	14	1	45	24	21	44	6	00	24	14	38
Ditto 25	—	—	—	—	—	1	35	24	20	10	6	00	24	16	28
Ditto 26	8	40	24	10	42	2	00	24	20	25	5	55	24	14	54
Ditto 27	8	30	24	12	33	1	50	24	23	39	5	55	24	18	11
Ditto 28	8	55	24	15	32	1	45	24	24	24	5	50	24	05	40
Ditto 29	8	30	24	13	26	1	30	24	21	13	5	45	24	13	29
Ditto 30	8	25	24	13	26	—	—	—	—	—	5	40	24	15	21

1814.

		at	h	′		Variation	°	′	″	
Mean of	Morning	at	8ʰ	32′	.....	Variation	24°	14′	33″	West.
Observations	Noon	at	1	39	.....	Ditto	24	23	17	
in Sept.	Evening	at	6	19	.....	Ditto	24	16	50	
	Morning	at	8	30	.....	Ditto	24	14	13	West.
Ditto in Aug.	Noon	at	1	39	.....	Ditto	24	23	48	
	Evening	at	6	57	.....	Ditto	24	16	31	
	Morning	at	8	41	.....	Ditto	24	13	29	West.
Ditto in July.	Noon	at	1	42	.....	Ditto	24	23	44	
	Evening	at	6	58	.....	Ditto	24	17	09	
	Morning	at	8	44	.....	Ditto	24	13	10	West.
Ditto in June.	Noon	at	1	30	.....	Ditto	24	22	48	
	Evening	at	6	52	.....	Ditto	24	16	29	
	Morning	at	8	45	.....	Ditto	24	13	12	West.
Ditto in May.	Noon	at	1	44	.....	Ditto	24	22	13	
	Evening	at	6	38	.....	Ditto	24	16	14	
	Morning	at	8	45	.....	Ditto	24	12	53	West.
Ditto in April.	Noon	at	1	48	.....	Ditto	24	23	53	
	Evening	at	6	29	.....	Ditto	24	15	30	
	Morning	at	8	52	.....	Ditto	24	14	29	West.
Ditto in March.	Noon	at	1	52	.....	Ditto	24	23	08	
	Evening	at	6	11	.....	Ditto	24	15	33	
	Morning	at	8	47	.....	Ditto	24	14	50	West.
Ditto in Feb.	Noon	at	1	52	.....	Ditto	24	20	58	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
	Morning	at	8	52	.....	Ditto	24	15	05	West.
Ditto in Jan.	Noon	at	1	53	.....	Ditto	24	19	03	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
1813.	Morning	at	8	53	.....	Ditto	24	17	39	West.
Ditto in Dec.	Noon	at	1	51	.....	Ditto	24	20	30	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
	Morning	at	8	40	.....	Ditto	24	17	17	West.
Ditto in Nov.	Noon	at	1	54	.....	Ditto	24	20	24	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
	Morning	at	8	45	.....	Ditto	24	15	41	West.
Ditto in Oct.	Noon	at	1	59	.....	Ditto	24	22	53	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
	Morning	at	8	53	.....	Ditto	24	15	46	West.
Ditto in Sept.	Noon	at	2	02	.....	Ditto	24	22	32	
	Evening	at	6	03	.....	Ditto	24	16	04	
	Morning	at	8	44	.....	Ditto	24	15	55	West.
Ditto in Aug.	Noon	at	2	02	.....	Ditto	24	23	32	
	Evening	at	7	05	.....	Ditto	24	16	08	
	Morning	at	8	37	.....	Ditto	24	14	32	West.
Ditto in July.	Noon	at	1	50	.....	Ditto	24	23	04	
	Evening	at	7	08	.....	Ditto	24	16	43	
	Morning	at	8	30	.....	Ditto	24	12	55	West.
Ditto in June.	Noon	at	1	33	.....	Ditto	24	22	17	
	Evening	at	7	04	.....	Ditto	24	16	04	
	Morning	at	8	22	.....	Ditto	24	12	02	West.
Ditto in May.	Noon	at	1	57	.....	Ditto	24	20	51	
	Evening	at	6	40	.....	Ditto	24	13	47	
	Morning	at	8	31	.....	Ditto	24	09	18	West.
Ditto in April.	Noon	at	0	59	.....	Ditto	24	21	12	
	Evening	at	5	46	.....	Ditto	24	15	25	

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.	
		Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Oct.	1	8ʰ	25′	24°	12′	05″	1ʰ	40′	24°	23′	48″			
Ditto	2	8	30	24	12	46	1	30	24	23	07			
Ditto	3	8	35	24	14	25	1	50	24	22	05			
Ditto	4	8	40	24	14	00	1	50	24	20	57			
Ditto	5	8	35	24	14	34	1	50	24	22	47			
Ditto	6	—	—	—	—	—	1	40	24	19	53			
Ditto	7	8	40	24	12	42	1	45	24	21	41			
Ditto	8	8	35	24	12	17	1	40	24	22	10	Not observed.	Not observed.
Ditto	9	8	40	24	14	28	1	45	24	20	59			
Ditto	10	8	45	24	11	16	—	—	—	—	—			
Ditto	11	8	45	24	11	55	1	40	24	18	56			
Ditto	12	8	40	24	14	41	1	45	24	23	38			
Ditto	13	8	50	24	13	23	1	30	24	22	07			
Ditto	14	8	40	24	14	23	1	40	24	20	44			
Ditto	15	8	30	24	14	50	1	50	24	22	26			
Ditto	16	8	30	24	12	56	1	35	24	23	21			
Ditto	17	8	40	24	15	13	1	40	24	21	58			

Rain fallen $\begin{cases} \text{Between noon of the 1st Sept.} \\ \text{Between noon of the 1st Oct.} \end{cases}$ 2·041 inches.

Sept. 4.—In deducing the mean variation the noon observation is rejected.

Sept. 12.—The whole of this day's observations are rejected, on account of the remarkable difference of the variation.

Sept. 28.—The evening observation is rejected, on account of the unusual smallness of the variation. On that day three currents of air were observed, nearly in N., S., and E. directions. The wind settled easterly, and blew hard for some days.

Comparison of the Variation in the Years 1813 *and* 1814.

		1813.			1814.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	25″
	Noon	24	21	12	24	23	53	+	2	41
	Evening..........	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	22	13	+	1	19
	Evening..........	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening..........	24	16	04	24	16	18	+	0	44
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening..........	24	16	43	24	17	00	+	0	17
Aug.	Morning	24	15	55	24	14	13	−	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening..........	24	16	08	24	16	31	+	0	23
Sept.	Morning	24	15	46	24	14	33	−	1	16
	Noon	24	22	32	24	23	17	+	0	45
	Evening..........	24	16	04	24	16	50	+	0	46

Article XI.

Isaaco's Journal of a Voyage after Mr. Mungo Park, to ascertain his Life or Death.＊

I, ISAACO, left Senegal on Sunday, the 22d day of Tabasky, in the afternoon; came to an anchor at the foot of the bar; passed the bar next morning, and had nearly been lost; got on board the George; sailed in the night of the 23d from the roads, and anchored at Goree the 24th, at four *p. m.* On my arrival there, found that some of my things had been stolen; signified to the Commandant of Goree my intention to stop my voyage until my stolen goods were restored; the Commandant sent me again on board the George, and ordered the vessel to return to Senegal, that I might there make my complaints to the Governor. We were nine days at sea, with heavy weather; and not being able to reach that place on the tenth day, we returned to Goree.

The Commandant, on the Friday after my arrival, sent a courier to Senegal about my stolen goods. The courier returned on the Friday following, and brought them to me. I left Goree the same day, in the afternoon, with the George, for Gambie. Next day, in the night, arrived at Yommie. On Monday arrived at Pitfrey; left Telfrey same day; passed Tan Crowwally in the night, and came to an anchor opposite a forest. On Friday weighed at night; anchored at Batingo in the morning; left Batingo about breakfast-time; anchored at about four *p. m.* opposite a forest; weighed in the night. Wednesday morning anchored; weighed anchor after breakfast; came to about twelve *p. m.*; got under way immediately after; came to after sun-set; passed Caour in the night; came to about four *a. m.* On Thursday got under way in the evening; came to Yanimaron at twelve *a. m.*; left Yanimaron in the morning of Friday; came to Mong La; left Mong La same day at sun-set; came to Marian Sound; found there Robert Ainsley; landed in the night, which made eight days from my departure from Goree; presented to said Robert Ainsley the Governor's letter to him. Robert Ainsley after that kept me with him five days; he gave me, by the Governor's order, one horse, one ass, and 20 bars of beads.

Left Robert Ainsley on Wednesday morning for the village of

＊ Doubts having been lately expressed in some of the newspapers respecting the reality of Mr. Mungo Park's death, we take the opportunity of publishing the only authentic document on the subject which exists, the Journal of Isaaco, who was sent by the Governor of Senegal to ascertain the fate of this unfortunate traveller. This Journal was written originally in Arabic, from which it was translated into Joliffe, thence into French, and from French into English. We are not at liberty at present to state the source from which we obtained it, though we have the most complete evidence of its authenticity. It may be sufficient to say that the MS. was sent from the coast of Africa to a scientific gentleman in London, for his private satisfaction. It appears to have been very badly translated, and is in many parts scarcely intelligible.

Cataba; sent same day my people with the above horse, ass, and beads, to Giammalocosto. On my arrival before Cataba I gave him one musquet, and one string amber, No. 4, which was distributed to his people in the evening; same day left King Cataba; arrived at Giammalocosto after sun-set, where I met my people. Left Giammalocosto Friday morning; slept at Tandecounda. Started from thence Saturday morning; slept at Guendi. Started from thence on Sunday; crossed a rivulet; stopped in the day under a tamarind tree, close to the village Sandougonmanna; sent to King Sallatigueboure, in that village, five bars tobacco (ten hds.); went from thence, and slept at Obimanna; gave to Mansancoye, the King, two bars scarlet and two bars tobacco; gave to my landlord three bars tobacco, to the King's son one bar scarlet. Started from thence in the morning. Next day stopped at twelve *a. m.* at Canope; slept at Coussaige; found my family there, being chased by the army of the Bambara; staid at Coussaige two days; gave Martafody, Chief of the village, three bottles of powder; left Coussaige in the evening; arrived at Mountogore in the morning, where had resided my family, which I had brought back with me from Coussaige to Mountogore, where I found my mother, in which village I staid, and all my family, about one month and a half. Forty-six days after left Mountogore, with all my family, after having disposed of such property as I could not carry with me. Stopped at Moundoundoo after crossing three rivulets; kissed there the Chief, named Mamandon, who killed me a sheep; gave him one bottle of powder; started in the morning; stopped at Couchiar at twelve *p. m.* under a bark-tree, where I passed the rest of the day. At four *p. m.* filled my leather bags with water; started from thence; travelled all night; came to Saabie at three *a. m.*; this place is inhabited by Marabouts; staid there two days, where I found a relation of one of my wives; gave him one bottle of powder and three pagnes; started from Saabie in the morning; at twelve *a. m.* stopped at Ioumajaoury, and slept there; started in the morning; arrived at Tallimangoly at twelve *a. m.*; found there a relation, who killed a sheep; gave her three grains of amber; slept there; started next morning; stopped at twelve *a. m.* at Banisirarjla, where I found the King of Boundon with the army of Bambara; went to salute him, and gave him ten bottles of powder, 13 grains amber, No. 1, two grains coral, No. 1, and one box tin gilt to the King's goldsmith, four pagnes to his first valet, one pagne worth one piece bafts, to the Chief of the village ten bars powder (two bottles); slept there two nights; started in the morning on my voyage, while the above army started on the opposite way; stopped at twelve *a. m.* at Cambarja, being very hungry; started in the evening; slept on the road; passed about eight *a. m.* at Gnery Sangnong-agy; received there some peas without stopping; Stopped at twelve *a. m.* at Doogay; started next morning at twelve, *a. m.*; stopped at Dahcaba; started in the evening, and slept at Bogoldande; started next morning; stopped at twelve *a. m.*

at Samacollo, where some singers came; gave them some trifles; had to dispute to save one of my dogs from being killed; left the place next morning; arrived at twelve *p. m.* at Soumbourdogo, and slept there.

Arrived next morning at nine *a. m.* at Delbon; Saloumon, my friend, gave me two sheep; gave him two bottles powder. The same Saloumon told me he would keep me company to Sigo; gave him ten pagnes to give to his wife to buy what might support her until his return; started with him from Delbon, with my family; crossed the Fitterni; stopped at the other side, at a village likewise called Delbon; bought there two sheep, and corn to make my couscous; stopped there three days for that purpose; started from there the first day of Reaky Gammon. Monday arrived at twelve *a. m.* at Diggychouncoumi, residence of the King of Boundon; staid there four days; killed for me two sheep; bought one sheep; gave my landlord two bottles powder; to Almany Sego two bottles of powder. Started in the morning; slept at Iaboucire, last village of the kingdom of Boundon; left it next morning; passed at Goutoumbo; slept on the road. Next morning at nine *a. m.* stopped at Dramani, in sight of St. Joseph's Fort, of Gallam; staid there five days to settle a palaver about one of my wives; received a bullock and four sheep; gave Enchoumoni 14 bls. in amber and powder, to the people $1\frac{1}{2}$ bottle powder and two bls. amber, to the Chief of Gallam two bottles of powder and two flints. Started in the morning; crossed Choligolic and Iaminchole, two rivulets; arrived at twelve *a. m.* at Moussalo; slept there; was well treated by the Chief; gave him two flints and 30 loads of powder. Departed very early; arrived at Tamboncani at about nine *a. m.*; found a Moor with a fine mare, which I bought with the goods returned me in the palaver I had about one of my wives at Dramani; saw there a large fort, built by the King of Bambara; arrived at twelve *a. m.* at Similouta; slept at Guiachalel, at the house of Amadifaci Chief; stopped there the next day, on account of one of my slaves running away, whom I got back again. Departed early; crossed Senegal river at Sitoncouti on the Moor's side, bought a sheep; slept there, and was well treated.

Departed early; stopped at nine *a. m.* at Couton; slept there; found only one woman; the men had followed the army; departed early; crossed Cholibinni; stopped at Chalimacouna, where I staid two days; Ourigiagui, the Chief, received me well, and killed one bullock; gave him one bottle of powder; departed before day-break, at the lake of Doro, to take water; stopped at Midina at nine *a. m.*; staid there twelve days, to wait the return of one of my fellow travellers. Not hearing any thing of him, I sent a man after him, because he took away my mare with him. The man brought me back my mare and musquet. I was well treated by the Chief, and village; received five sheep from them; gave the Chief one bottle, and the village $1\frac{1}{2}$ bottle, and bought a sheep. Thus completed the three moons from my departure from Montagou.

Departed early; crossed Kirgou, a river full of hippopotamus and alligators: arrived at twelve *a. m.* at Cougnacary, formerly the capital city of the kingdom of Cassoo, but now occupied by Bambaras; received one sheep; gave one bottle of powder and five flints; slept there; started early; went round, and crossed again Kirgou; passed at nine *a. m.* at Maritoumani; passed a large rock called Tap-pa; arrived at twelve *a. m.* at Camatingui, after crossing five rivers; staid there two days; received one bullock and one sheep from the Scracoulies residing at Cassoo; gave Marimoussa, the Chief, half a bl. and ten grains amber. One of my slaves was there redeemed, and I received another in his place; met, while there, the King of Bambara's messenger; gave him half a bottle powder. Departed early; crossed Gany between two rocks; arrived at twelve *a. m.* at Lambatra; slept there, surrounded by rocks. Started early; had to ascend large mountains after taking water for provision; at about twelve *a. m.* arrived on the top of a mountain, where my people, who were before, were assailed by the bees, which scattered all my people and my beasts. After they were a little appeased, went back to the spot to collect my things that had been thrown about by the beasts of burden; found one of my asses dead, being stifled by the bees getting into the nostrils, and one of my men almost dead; had to give him something to bring him to life; slept at the foot of that mountain, under a monkey bread-tree; departed early, at about nine *a. m.*

Met on the road one of the King of Bambara's men sent after me; sat down under a tree with him, who told me he was sent by his master towards me to tell me that if he met me at Cougnacary he would procure me provisions, and keep me there to rest myself; but as he met me on the road past Cougnacary he would lead me to the first village to get me provision, and rest myself; to which I agreed. Goundouquide, arrived at four *p. m.* at liguitingyalla. On my arrival there I told the messenger I was going to send somebody to the King to let him know of my arrival. I then sent a man, and told him, on his arrival at Giocha, where the King was, to give to Sabilli, Chief of all the King's slaves, 13 grains of amber, No. 1, one pair of scissars, one snuff-box, one looking-glass, &c.; to tell Sabilli that I sent him those things, and let him know of my arrival. After this man's departure, I sent another messenger, with one round half dollar and one grain amber, No. 1, and told him to go to the same place, and endeavour to see Alasano-bouare, one of the King of Sego's messengers, and tell him I sent him that grain of amber and that piece of silver as a mark of my being near him, and not to leave the place before he sees me. I had learnt his arrival by a caravan of slaves I had before met on my road. After I had sent those two messengers, in the evening the King's messenger told me, " I am going away, but shall give orders to the first village to receive you, and give you provisions and all assistance, and wait there for further orders." I then slept. In the course of the night the Chief of the village where I was ordered to stop had

sent to tell his son, residing at Iiguitingalla, where I was, that it was useless for me to go any further. Next morning his son said to me, " You need not go any further; for my father sent to me an order to furnish you here what you may want, and you may stay here. I told him that if I staid here I and my family would die of hunger and thirst, and that I would go where I was ordered, unless I was stopped by force. I then started, and arrived at twelve *a. m.* at Marybougou, where I was ordered to stop by the Chief, named Foutamasso, who sent me to get lodging at his brother's house. When I came to his brother's I was refused lodgings. I then went under a monkey bread-tree, and made a halt.

The Chief came to me, and told me to stay here. I observed to him that I could not stay, as water was very scarce. He then gave orders that no one in the village should draw water, that I should have what I wanted, and have no reason to complain of that article. I took that opportunity to give drink to my cattle, horses, asses, hogs, dogs, sheep, &c. and filled my skins. Being ready to depart from thence, the two men I had sent from Iiguitingyalla arrived. The one I had sent to the Chief of the slaves told me that man received my presents, and said that I wish to be his friend, to which he had no objection. The other told me the King of Sego's messenger said that I may be assured he would not leave the place before he saw me, according to my desire. I had in my caravan a man I met at Dramani from Senegal, who had some friends in that village of Giocha, who sent to tell him to take his goods away from mine, and put them aside : for in case I should be plundered, his goods would be lost if found with mine, to which this man objected. This gave me a proof of his good intention ; and at the same time I was convinced that something was framing or planning against me. I then forced this man to take away his goods from mine, as it would be unjust he should suffer on my account. I then immediately put my arms in order, and well loaded, and placed myself against the tree with two double-barrelled guns and one musquet, in waiting for what should happen. While I was in that posture of defence comes a messenger to me from the King, who was the same I first met. He told me that as there was not water enough for me I should go on further, to Wouassaba, where I should not want. I started in the afternoon, and arrived at sun-set at Wouassaba. On my arrival there the messenger told me there was a house to put my things in safety. He then wanted to separate my people from me, and distribute them in the village ; to which I objected. I took lodging with all my people in the yard belonging to the house appointed for my residence. The Chief of the village said to me, " Send your people with me, and I will give you a bullock." The King's messenger spoke a little while with him separately. He then came back, and said he could not give me the bullock, as the cattle were very far off, with the King's cattle. When the messenger saw me settled in the yard, and disposed to spend there the evening, he left me, and went away.

When I was sure of his departure, I sent another courier to Guicha, and told him, on his arrival at Guicha, to go and see Madeguigou Marabout, who will introduce you to Sabiti, and give him seven grains amber ; and tell him, Sabiti, to go and tell the King his master that wherever I go I meet some of his people, who make me stop at one place or the other ; and that my intention is positively to go to him. My courier came back to me the next day, and said that Sabiti let me know that the King his master's will and pleasure is that I should stay where I was that day, and come to him the next day ; which I did accordingly. Next day the King sent me a messenger, to order me to go to him. I left my family and all my friends there, and went with three horsemen and four foot, and went on. I had sent before me a courier, with five grains of the largest amber, No. 1, with orders to wait there for me. Arrived about three *p. m.* on Tuesday at the back of the village of Guicha ; I was met by the boy I had sent before me with the five grains of amber, who said to me in the ear, " Where are you going ; we are betrayed ; do not say you are going to Sego, for our lives depend on it." I told him I was sent by the Governor of Senegal to Sego, and to Sego I must go, unless I am stopped by death or force, and that I would not deny it. I then entered the village, and went straight to the King's door, followed by the King's messenger, who had followed me from Woussaba. Before the King's door I alighted ; his messenger told me to stop, and wait there ; he went in, and came back immediately, and told me the King was asleep. The King's Guards took possession of me, and lodged me in the guard-room with them, along with my people : this was about sun-set. Not a single soul came to see me, not even my nearest relations, except a Griot woman, who came to comfort me in my distress. This Griot woman, immediately on leaving me, went to the messengers, which I learnt afterwards of Sego, and said, Oh me ! my back is broke. The messengers asked her, How ? She said, Because Isaaco is here, and they are going to kill him. Not learning any thing from Sabiti, Chief of the slaves, I sent my same boy to Madiguigou Marabout, to be introduced to Sabiti, and give him the five grains of amber. I sent another man to my landlord, where I always resided when in that village, to tell him, with my compliments, my surprise at not seeing him since my arrival. He sent me word that he was happy to hear of my arrival, as nobody had before informed him of it.

Not being well guarded in the night, I sent the man who was advised to withdraw his goods from mine to the Sego messengers, and inform them of my being there ; went in the night to my landlord, and gave him one of my women's necklace, with nine grains amber and seven grains coral. From thence I went to Madiguigou Marabout, and told him I was sent on a mission to the King of Sego with some papers, in order to facilitate my journey in search of a white man, who had gone in the interior of this country long ago. From thence I went to Sabiti, and told him the same thing. After-

wards I went back to the guard-house, and laid myself to sleep, while the Guards were amusing themselves in drinking, dancing, and singing. My slumber being disturbed by my uneasy mind, I awoke, and found all the Guards gone. I went out of the house, and returned to sleep, but found it impossible to do so. I then heard the steps of many horses in the street, which I presumed were going to Sabiti. Next morning early I sent another man to the messengers of Sego, to let them know of my critical situation, and of not hearing from them at the time that I heard they were on their departure, asking whether they intended to go and leave me in such distress. The messengers sent me back the man, asking why I did not follow this time the same road I had followed on the other voyage. I sent back the man, to tell them as the two countries were at peace I thought it secure to travel through it. However, since they were determined to go without me, they might do so. Whether I should be released or dead, they should hear it soon enough at Sego. That Mungo Park promised a present to the King Mansoung; and Mungo Park not returning, the Governor of Senegal and Goree gave me the present which I am now the bearer of for Mansoung. The messengers sent to tell the King that they heard I was at Guicha, bearer of a present to Dacha, which Mungo Park had promised to Mansoung, the present King's father. Mungo Park not returning to his country his relations had given me that present to carry to Sego; and in case I should wish to go back, they (the messengers) advised him, the King Peguicoroba, not to let me go back; and if I wish to go on my voyage, they beg he, the King, would give me some trusty persons to go along with me. In the mean while a Marabout, named Massataneuagui, came and told me all about how I had been arrested with intention to put me to death, and that Sabiti had some how saved my life; but I thank God only for my life, although the Sego messengers and Sabiti had been a means of my safety. He advised me to give the King's only child something. I went to the Prince with this Marabout, and gave him half a piece white cotton, two grains amber, No. 1. I went back to the guard-house, where I passed the night following.

Next morning my landlord went to the King, begging permission, as every thing was settled, and appeared favourable on my side, to take me to his lodging; to which the King consented. He then came to me to let me know that I was free, and I went with him to his house, with those I had brought with me. From thence I went with Ionmanchong, my landlord, and my people, to the King. On arriving, I presented to him a fine tin box. The King then spoke to Sabiti, and said, What is the business? Sabiti said, This is our old friend, and a good man. My landlord said the same. The King then said to me, Here is your box, keep it; what else you have brought into my country I shall keep. Return to the place where you first started, and travel on your mission by the same road you travelled first with the white man; but your goods, and every thing else belonging to you, I shall keep. I know what

you have is destined for the King of Sego. I said, I might, it is true, have travelled by other roads, where you never would have heard of me; but on my way hearing you lived with the King of Sego in peace and friendship, I took it upon me to travel through your country in security; the King said, What I said to you is enough. I then left the house with part of his slaves, and went to my lodging. I immediately completed 60 bars in powder and amber, the horse Robert Ainsley bought me, with the same tin box he refused, and three ducks. I went with my landlord with those presents to the King, and presented them to him, which he accepted. In the presence of the King I gave Sabiti one bottle powder, to his Griot one snuff-box. The King then told me, Now if you choose I shall send you a man who will conduct you straight to Sego. I observed to him that I could not go now, because if I did whoever would see me would think that I deserted from him. I shall stay here, and rest myself awhile. The King then said to Sabiti, You see that man; he appears to be a man of great courage; if he had been a weak spirited man he would have run away, and left his things in my hands. I then went home; spent the day and night; next day I went with my people to Waassaba, to fetch my family and things, where I staid two days, but thought proper to go back to the King, being afraid of some underworks against me. When I came before him, I told him that I came back to swear fidelity and friendship to him, and that whenever I should go backwards or forwards to Senegal or Sego that I should always put into his country and see him; but that I should wish also he would swear to treat me well, and be my friend and protector, should he be at war with the King of Sego. He then sent for Chiaman, the eldest son of the Royal Family, who swore the same to me in the King's name. I then swore likewise before them what is above stated. After swearing, Chiaman told me to give him, by way of cementing our oaths, a handsome gun or a coussatu (shirt). I told him I had none at present; but gave him my word, if I should go back to the country of the white men, on my return I would bring one of those two things. I stayed in the village all that day.

In the mean while I wrote a prayer (*gris gris*) to a man who gave me a bullock, which I carried to my family and people at Waassaba. Slept at Waassaba, next morning killed the bullock, and passed that day. Next morning Iagui, Chamoni's brother, sent me word to wait there for him. I then immediately sent my family and things by another road, and waited the arrival of Iagui. Iagui came, and presented me an ass loaded with dried couscous, to help me in my travels. I gave him half a piece of fine cotton, three bottles powder, two looking-glasses, two snuff-boxes. Iagui left me, departed same day for Toucha to take leave of the King, informing me that by and by he will give me the promised man to conduct me (between Waassaba and Toucha there are seven small rivers to cross). The King gave me a man, named Mouroucoro, who went on foot. The King then shook hands with me, saying, I bear you

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no evil now, but did so once, because you conducted white men to Sego, and never passed here to let me have something from them, while every body shared the white men's generosities. I took my leave of him, and went to Checouray, Chaman's village, where I found my family and things about the evening. Staid there two days; Chaman killed a bullock; gave him one pagne worth two pieces baft, one bottle powder, 20 flints, one bar scarlet; departed in the evening; stopped at Checouray Sambabile, Chaman's other brother (the two villages bearing the same name). Sambabile gave me some corn and one sheep; gave him one blue pagne, one stripe ditto, one bottle powder, 20 flints, one bar scarlet, which pagnes I got on the sale of three slaves I sold to help me in my expenses; staid there two days, and departed.

In the morning arrived at twelve *a. m.* at Tiallacorro, where resided Madifoulane, the King's son, to whom I had given half a piece fine cotton and two grains amber. He gave me some corn. Madimariam Marabout killed me a bullock; gave the Marabout one bottle powder. Next morning started from thence; passed three villages; arrived at three *p. m.* at Col La; received cooked victuals from the village; gave two flints. Next morning, Wednesday, departed, and arrived at twelve *a. m.* at Amadifatouma-bougou, the last village belonging to the King Tiguicoroba, six moons after my departure from Senegal; bought there an ass. Having before me a great forest to go through, and uncertain of the right road, I hired four men to conduct me; departed next morning, and crossed a small river near the village, being in the forest at noon; came to a large muddy pond, where the hogs could not cross over. The guides showed me a better road, which enabled us to pass safely. At about two *p. m.* stopped awhile, where had been formerly a village; after sun-set found in our way a large turtle, which we killed, and passed there the night. Departed next morning; passed at about ten *a. m.* at Sarini, a destroyed village; stopped there awhile; the four men left me to return home, being afraid of going further; gave them half a bottle powder and ten flints. Being uncertain of my ways, and being displeased with their proceeding, I said I would sooner go back than be left alone in such a forest. They told me to follow the way they showed me, straight along, and not turn on the right or left; that I should soon find a village inhabited.

I then went on, and found on my way the road the King of Sego's army had taken nine years ago; further on found a small spot with water: being very thirsty, spent there part of the day; further on found a large pond; stopped there under a tree, and slept there. Started early; arrived at noon at the lakes of Chinohare and Tirin, which are never dry, where the King's army always stops to take water after dinner. Departed, and arrived about five *p. m.* before another lake. Went on, came to Giangounte after sun-set, and stopped there five days, on account of one of my people being sick; received first night a little provisions; next day they killed me

a bullock; on the third day the King's people came there, the village gave them a bullock and a sheep, which I killed myself; they gave me a quarter of each for my provisions. On my arrival there, I thanked God for my escape. This village is surrounded by a large mud wall, and is well fortified. I presume the village to be well secured against any attack.

Found one of the hogs so large and fat that I could not carry it any further, and was obliged to leave it there. I told the Chief of the village, who did not wish to keep the hog, that I found it difficult to carry it any further, that I would leave it with him, and he might do with it what he pleased, for the village belongs to my King, so did the hog, and that I was sure he would take good care of it. Departed early; arrived at noon at Fabougou; left it after dinner; arrived after sun-set at Giongoey; staid there two days; departed early, and arrived at ten *a. m.* under a tree before a lake called Son-ni; crossed the lake; stopped awhile at Ionniguita; arrived at Gommentora, where we spent the night; received a sheep; departed next morning; arrived at ten *a. m.* at Wattre; went on in the evening; passed a large open field, dangerous for travellers, on account of the Moors passing very often there; travelled the rest of the day and all the night; arrived about three *a. m.* at Toucha. On my way from Gommentora to Toucha there is a tree grown on the top of a dried stump of another large tree, the wood of which is employed in the composition of our gunpowder; there is a rock near that tree forming a kind of a pyramid, and a large rock standing on the top of the pyramid. On my arrival at Toucha found that the case the Chief, Governor Maxwell, had given me, which contained some looking-glasses, beads, my fine shirts, and my wife's bracelets, and which was carried by my nephew, was missing. I asked the boy where it was. He told me that, being fatigued on the road, he had given it to a man who had followed them from Iocha; and that the man must have stolen it, as he did not see him go.

Left my family; received there next morning ten moules corn; went to Camicon at noon; received a sheep, milk, and corn; the Chief named Fiong; departed in the evening, and passed Sidong; arrived at sun-set at Sannanba; slept there; at which village I had left my wife and sister on my voyage with Mungo Park; found them there on my way to Senegal, where they had waited my return the whole time. I said to them, You are my wife and my sister; tell me if you have heard any thing of Mungo Park. My wife told me that she saw Athagi Beraim, who told her that Mungo Park had died in the country of Douissa, and that he had seen the boat in which he was when he died. Yammi Marabout gave me a bullock; so did Moullini and Guiniba; Facow, the Chief, gave me one also, with corn; received two sheep from Alhagi, one ditto from Fatime Boubou, one ditto from Amadi Bini Doucoura, three ditto from Dimba Soumare.

The ninth day after my arrival there the hog I had left behind

was brought here ; received from Moutini one ass ; gave to Amadi Binni one musquet and ten cubits of white cotton, to Guami half a bottle powder, to my sister ten dollars and one pagne of muslin, to the wife one bottle powder and 20 flints, four days after the arrival of the hog, being the 13th day of my arrival there, and the seventh moon of my voyage. On Saturday I departed early with my people, and ordered the hog to be brought along with me by the same people ; passed Boromba ; took water at a large fountain ; passed Bancoumalla, after crossing a large lake ; stopped and slept at Sirteerra ; lodged with a Moor named Babamirini, who killed me a sheep ; received from Manchia, the Chief, one sheep ; gave them 20 load powder and ten flints ; departed at sun-rise ; arrived about two o'clock *a. m.* at Counnou, where there is but one well for the village, and three fine large Doualli around it ; found there the King's army.

There is on the east of the village a large tree full of bats, one tree in the west likewise full of them ; but what is most extraordinary is that those of the east go to the tree in the west, but those on the west never go to that of the east. The army departed about three *a. m.* and I about day-light ; on the road met the rear-guard on its way to join the army ; arrived at four *p. m.* at Gargnia ; slept there in a large village on the east ; there is a large lake where they take water, and two large trees on each side of the door to enter the village ; met there a caravan from Cancari ; received from them cotta. Started early ; arrived at ten *a. m.* at Didougou ; slept there ; the people of Gargnia had brought hither the hog, and gone back. The people of this village being out in their fields, I was obliged to wait next day to have the hog carried ; received three fowls ; gave three loads powder ; each village was obliged to give me four hands to have the hog transported. At Sannamba left there the thief, fearing the King would kill him. Departed early, passed Issicora, and five villages deserted ; slept at Yamma at four *p. m.* ; staid there three days, lodged at the house of Boyamodiba ; he killed me a sheep ; gave him two bls. scarlet ; a woman in my caravan, who had come with me from Moundogou, and had been redeemed there, found her husband, who gave me a sheep and 100 collas. Departed early ; arrived at noon at Yamina, on the border of the Niger ; wanted to embark in a canoe, but the rain prevented. At about four *p. m.* at Mognongo, on the other side of the river ; after passing nine villages, the river there is very wide. Departed early in the same canoe ; arrived at Samman at noon ; lodged with Gianguiana, where we stopped formerly with Mungo Park, and where Mungo Park lost three white men by diseases.

At four *p. m.* started, and went after sun-set at Segocorro, on the opposite side of the river from Samman, after passing four villages ; lodged with Sego Somma. This village was formerly the residence of the Kings ; but to this day, when the King wishes to go to war, he always goes there to make his *gris gris* and prepare himself ; and

when they take a king, or a prince, or a man of high rank, they keep him confined, and in the moon of fasting he is brought there, and placed in a house appropriated for that purpose. He is then killed there by cutting his throat, and there is not a moon of fasting passed away, but there is always some one killed there. For the space of one week after those executions, no body whatever is allowed to pass before that house, named Cognoba, with shoes or caps on; when the man is killed they let the blood be spilled there, and then the body is carried into the open field, and left there to the wild beasts.

Departed early; passed Segobougou, Segoucoura, Douabougou; arrived at about eight *a. m.* at Segochicosa, the residence of the king of Bambana, named Dacha, on the Monday 11th of the Moon. This town was built by his grandfather after his rebellion; who driving the king from his dominions he was proclaimed king himself. Himself and his party were slaves, they rose in a mass, got the better of their masters, drove them away: and this grandfather of Dacha, being the greatest warrior among them, was proclaimed king (his name was Wollo). Lodged with Guiavi, one of those attached to the king. Some one went in the evening to let the king know of my arrival; next morning the king got on horseback, and said, I am going to Douabougou: tell Isaaco to come there and meet me; but the rain coming on he was obliged to return back to his house. After the rain, he sent me word to go to see him at his lodging, to bring along with me the hogs in the same condition I had brought them, and in the way they were tied for travelling. On my entrance in the first yard I met a guard of forty men, young, strong, and without beard; in another yard saw a great many armed men under a shade. A little further on found the king sitting, with four sabres fastened to his back, which were given him by Mungo Park; dressed in his military coat, which he is obliged to wear when his army marches, and until it returns, when he takes it off; at other times he dresses in blue or white cotton or silk, and has great many *gris gris*, covered with plates of gold and silver, sewed up all about his dress. I sat down on one side of him and my landlord on the other side. After the usual salutations I laid before him the drum, the two blunderbusses, the bed, the one dog, (the other dog I forgot to mention ran away from me after leaving Mariamcounda), the two hogs, the scarlet. I said to him, Maxwell, governor of Senegal, salutes you, and gives his best compliments to you; here is the present Manchong your father asked of Mungo Park, and which he promised to send him: he asked me if the governor was well, I said, Yes, he is well, and has desired me to beg you would give me your assistance in my endeavours to go and see what has become of Mungo Park; and know positively whether he is dead or alive; and give me a vessel to facilitate me in my voyage, and the governor will reward you for so doing. He said what does the governor mean to give me? I said, For giving me all the assistance you can, the governor

will give you 200 bars.   He said, How can the governor give me
that sum being so far from me.   I said, He is from you here it is
true, but I am here to represent him : he then accepted my offer.

I arrived there the 11th day of the moon, and staid to the end
of it.   The king killed me a bullock.   The first day of the moon,
being the day I intended to depart, the prince of Toomboucoutou
came to Sego ; the king went out to meet him ; he said to the
king, that he came to let him know (he being a friend of Man-
chong) that he was coming here to get the wife promised him.
The king said to him, Why have you permitted the people of your
county to plunder a caravan belonging to my county, and did not
prevent it ? (My landlord lost in that caravan 700 grains of gold
and one slave.)   And why did you yourself plunder another caravan
of mine ? When the king came out of the door to speak to this
prince, he ordered out 600 of his guard, all young men without
beard, and almost naked, with each his musquet ; the king then
got up and went to his house, loaded the guard after unloading
their musquets in the air.   The prince when at his lodging con-
sidered he was critically situated, and that by his bad behaviour
the wife promised him had been given to another, and that the
people belonging to the caravan he had plundered had followed
him to the king : he sent to the king three horses, and to all the
chiefs half a piece white cotton.   Next day the king sent back the
ambassadors who had come from Joucha with his ambassadors.
Next day he went to Impibara.   I went the next day there to meet
him.   We staid there nine days.   Being displeased with such
delay I wanted to depart.   Some one went and said to the king, I
was angry and was going to depart   He said, Tell Isaaco to stay,
I am going to Banancoro, we will go together.   He next went to
Banancoro, and I staid ; he then sent me a courier to call me.   I
went there to Inche's house, the king's slave.   The motive of the
king's journey there was to see one of his children.   He has six
children living ; he had three put to death there.   Among the
kings, their custom is when a male child is born on Friday, they
order his throat to be cut, which is done immediately.   At about
ten, *a. m.* the king sent for me, I went to him, he then ordered
part of the present to be brought before him, in which were the
hogs, (the dog died a short time after my arrival) ; the hogs were
let loose before him which pleased him much.

Next day he gave me a canoe with three men, and I departed on
my voyage in the afternoon on Friday ; following the tide, passed
ten villages ; arrived at supper time at Sansanding, (that space is
two days' journey by land) ; slept there ; started at three, *p. m.*
by land, and arrived at sun-set at Madina : at Alihous, found there
Amaudy Fatouma, the guide Mungo Park took from Sansanding.
I sent to his house to call him, he came to me ; I asked him a
faithful account of what passed concerning Mungo Park.   His
first words were, They are dead.   I told him I came to see after
him, and I intended to look every way for you to know the truth.

He told me it was useless to make any further inquiry after him; for to look after what was irrevocably lost was losing time without any end. I told him, I am going to Sansanding, and you will come and meet me there to-morrow; slept at Sansanding. I sent back to Impebara the canoe. Amaudy Fatouma came at the appointed time to meet me, being the 21st day of the moon. I told him to let me know what passed to his knowledge concerning Mungo Park.

Amaudy's Journal.

He said, We departed from Sansanding in a canoe, the 27th day of the moon. We went to Selli in two days; the same village where Mungo Park slept his first voyage. Mungo Park bought a slave to help us to navigate the canoe, without landing we went in two days to Jenni; we gave the chief of the village a sieve of baft; we continued and arrived at Sibby, I forget in how many days; we arrived here without any danger. On passing Sibby, three canoes came after us with their weapons; being certain of their hostile intentions, we repulsed them by force of arms, and passed on. Came to Cabbara; on my passing there, three canoes came again to oppose our passage, we repulsed them by force as before; came to Toomboucoutou; on passing there we were again assailed by three other canoes, which we repulsed; passed Gouroumo, after passing seven canoes after us, which we likewise repulsed; we lost one white man, of sickness; there were then in the canoe only four white men, myself, and three slaves we had bought, making eight hands; each of us had 15 musquets apiece, well loaded, and always ready for action; passed before a village of which I forget the name, the residence of the king Goloijigi; after passing this village 60 canoes came after us, which we repulsed after killing many of the natives, which we had done in all our former engagements. In this last action we killed so many men of the enemy, that seeing our superiority, I took hold of Mastin's hands, and said, We have killed enough, let us cease firing. Mastin wanted to kill me, had not Mungo Park interfered. After passing Goloijigi a long way, we met an army very strong, all on foot, composed of the Poule nation, without any beasts whatsoever with them; we passed on the other side of the Jalliba, and went on without any hostilities.

On going along, the canoe touched on the rocks. An hippopotamus rose on the other side of the rock upon us, and was like to have destroyed the canoe; we fired on the animal, drove him away, and got with great pains our boat off. We came to anchor in the middle of the river before Caffo; passed there the day; departed in the evening. We had when we first embarked a great quantity of provisions of all kinds, the canoe being very large, and capable of containing 100 people: we had no occasion to stop at any place, neither did we; came to anchor near an island, found a great quantity of hippopotamus on shore; on our approach they

went in the water in such a confusion, that they almost upset our canoe; passed on all night. In the morning three canoes from Caffo came after us, which we repulsed; we came to a small island, and found there some natives, I was sent on shore to get some milk; when I got among the natives, I saw two canoes go on board to sell fresh provisions, fowls, and rice, &c.; when among them one of the natives wanted to kill me; he took hold of me as a prisoner; Mungo Park seeing what passed, stopped the canoes and the people, saying, If they kill or stop any man on shore, I shall kill you all, and carry your canoes with me; they then sent me on board in another canoe. Mungo Park then let them all go. A short time after our departure, 20 canoes from the same place came after us; when they came near they hailed us, and said to me, Amaudy Foutouma, how can you pass in our country without giving us any thing? I repeated the same to Mungo Park, and he gave them a few grains of amber and other trinkets, and they went back peaceably.

We came to a shallow part of the river, found on shore a great many of the natives sitting; on our coming near they got up; we presented our musquets to them; they ran off in the interior and went to their village. A little further on we found the river barred by the rocks, but there were three small passes. Coming near one of the passes, we saw the same people standing on the top of the rocks, which caused great uneasiness to us all, especially me; and I vowed before I passed there again to make great presents. We then went to a pass of less danger, and passed unmolested. We passed before Carmasse, came to anchor, and gave one piece of baft to the chief. We went on, and anchored at another village called Gourmon. I was sent on shore with 40,000 cous to buy milk, rice, and onions, which I did, and departed in the evening late. The chief of the village sent a canoe after us, which hailed us, I answered them; they said, the chief of the village sent us after you, to let you know that there is before you a very large mountain, and a very large army on its top, waiting for your coming, you had better be well on your guard. We immediately came to an anchor, passed there the rest of the day and the night. Next morning we departed; on passing before the same mountain we saw that army, being all Moors, with their horses, camels, &c. but without any fire-arms. They said nothing to us; we went on and entered the county of Haussa, came to an anchor. Mungo Park said to me, You are now at the end of your journey, I had engaged you to conduct me here; here you are going to leave me, but before you go, you must give me the names of all the necessaries of life, in the languages of the country I am going to pass, which I agreed to. We passed two days together without landing; during our voyage I was the only one who landed. We departed, and arrived at Yaour, where I landed the next morning, with a musquet and sabre to carry to the chief as a present, which I did; I had also three pieces baft, which I gave to

Allagi, Allhagibirou, and another whose name I forget, all three Marabouts; the chief gave us a bullock, one sheep, three jars honey, three jars butter, and four men's load of rice. Mungo Park had paid me what we had agreed for my pains, before my departure with him; I then said to him, I agreed to conduct you to the kingdom of Haoussa; we are now in Haoussa, and I have fulfilled my engagements with you. He gave me 700 cous, and ordered me to buy some provisions for him, which I did: he gave me five silver rings, some powder and flints to carry to the chief of the village, and say to him, It is to the king living near this village a present from the white man, who is going away and is taking leave of you and the king, which I did; the chief then asked Mungo Park if he intended to come back, Mungo Park said, No, I will not come back. We slept there next day, being Saturday. Mungo Park departed, and I staid on shore, and slept where I had landed. I went to salute the king at his village; on arriving there I found two horsemen, sent by the chief of the village to the king, who said, The chief sends us to tell you, the white men went away without giving you or me any thing, they have a great deal of goods, but I have received nothing from them; and this Amaudy Fatouma, likewise, has made a fool of us. The king then immediately ordered me to be put in irons. I was cast in irons and every thing taken from me. Next day some were for killing me, and some not.

They sent an army to a village called Boussa, near the river. Before that village there is a rock stopping the whole breadth of the river, one part of the river being very high; there is a hole where the water can pass, but very narrow; his army went and took possession of the top of this high rock. When Mungo Park attempted to pass, the people began to throw rocks and lances at them. Mungo Park defended himself for a long time; he had two slaves killed at the stern, after defending themselves long; and being overpowered by numbers and fatigue, and the current so strong, they began to throw over every thing they had in the boat. Mungo Park took hold of one of the white men, and they threw themselves in the water. Mastin did the same to the other white man. The natives persisted in throwing rocks and lances. The only man remaining in the canoe, a slave, said to them, There is nothing in the canoe but me, why are you so desperate; cease, and take me if you like; which they did. They took the canoe and the man, and went back to the King.

I was kept in irons for three months, when the King released me, and gave me a slave woman. I then went to see the slave taken in the canoe, who told me how Mungo Park had died, which I have related above. I asked him if any thing had been found in the canoe after taking it. He said that nothing was found but myself and a sword belt. I then asked where was the belt. He said it was with the King, *who made a sangle for his horse.* I then sent a Poule to get me the belt, by any means and at any price, and any

thing else he could find belonging to Mungo Park.   I left Madina, and went to Sansanding, and from thence to Sego.   On my arrival I went to Dacha, the King, and told him as above related.   He said he would have gone and destroyed that country if it was not so far.   He then formed an army, and went with it to Banancoro.   I followed him there.   I staid there with the King.   The army was sent to Haoussa, which, after passing Tomboocoutoo, they made halt at Sacha, and dispatched a courier to the King to let him know where they were, and that Haoussa was too far off for an army to go thither.   The King ordered them to go to Massina, a small country belonging to the Pauls, and take all their cattle, and return.   They did so, and brought with them a great quantity of cattle, after being out three months, viz. the van-guard, but the army did not return till the end of the fourth month.

The King was much displeased at the Chiefs of the army, and wanted to punish them for not going where he sent them.   They said they went as far as they possibly could, but the distance was too great.   It would have destroyed the army, and prudence dictated to them the hard necessity of returning.   We came all together back to Sego; from Sego I went back to Sansanding, and staid there four months.   The poule I sent came back, after a voyage of eight months, with the belt.   He told me he had bribed a young girl, slave of the King, to steal the belt, and brought it to me; he could get nothing else, as nothing remained after Mungo Park. I then went to Sego, and told the King what I had got belonging to Mungo Park, and that I was going back to Senegal. The King wanted me to spend the remainder of the season with him.   I said I could not; as my mission was at an end, I would not stay.

Amaudy Foutouma being a good and upright man, I had placed him with Mungo Park.   What he related to me, with oath, may be believed, having no interest nor any hopes of any reward whatever, nothing remaining of Mungo Park or his effects.   The relations of several travellers who had passed that same country agreeing with Amaudy's journal, the dangers I should have run, to no effect, in such a distant part, and my being certain of Amady's words: all these reasons engaged me to go no further; after obtaining the belt, I thought it best to return to Senegal.

Article XII.

Proceedings of Philosophical Societies.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(*Continued from* p. 311.)

Mineralogy and Geology.

The method of making correct observations becomes every day more prevalent in geology, and we every day acquire more correct notions respecting the strata of which every country is composed, respecting the general laws of their superposition, and of the organized bodies the remains of which these contain.

The rocky beds containing only fresh-water shells, so great an extent of which was discovered in the environs of Paris by MM. Cuvier and Brogniart, and which MM. Brogniart, Omalius de Halloy, M. de Serres, Daudebart de Ferussac, &c. have found in a great number of other countries, have particularly excited attention, and have induced naturalists to make researches in order to distinguish fresh-water shells from those of brackish and of sea water. MM. de Ferussac and M. de Serres have each given a memoir on this subject. The species alone, says the first of these gentlemen, can be brought as evidence, and not the genera; for most of the genera have both marine and fresh-water species. It is not even useless to study the varieties; for the same species, according to the observations of the same author, sometimes changes its form so much as not to be distinguished by one who has not attended to its different transitions; and the difficulty increases when we have fossil shells to determine, when the epidermis, the hairs, and all the other characters of little solidity, have disappeared.

There are species, especially among the *opercula,* that are capable of living in fresh-water, and which therefore are found most abundantly at the mouths of rivers; and we observe among the fossils traces of this habit; for our fresh-water banks in certain places contain a species of *potomide,* a genus of shells usually found at the mouths of rivers.

M. Marcel de Serres visited on purpose the salt lakes on the coast of the Mediterranean, to examine the shells that are found in them. He observed *paludines* very similar to those which form large banks in the neighbourhood of Mentz, along with which occur various sea-shells. A geologist who had confounded these *paludines* with the *bulimias* of our fresh-water beds, had concluded that these last are sea-shells, as well as the others. But M. de Serres corrects this

mistake, and shows that not only the species, but even the genus, is different.

This observer has endeavoured to ascertain the limits of this passage of animals and plants from salt to fresh-water, and the contrary. He finds that no animal nor no plant can resist a degree of saltness amounting to eight degrees. He has distinguished, both among animals and plants, those species which affect the sea-shore merely on account of the sand to be found there, and which can live likewise in other sandy places; those which are attracted to that situation solely by the salt, and which live likewise in interior salt lakes or marshes; and, finally, those which require the sea such as it is, and do not remove to any distance from it.

These observations show that it is not always easy to distinguish whether a shell belongs to salt or fresh-water; but they do not in the least diminish our certainty of the fact that immense beds exist containing only shells well known to be fresh-water ones. They even explain why we find these shells scattered in marine beds.

M. de Serres places the *lignites*, or bituminous wood, among fossils, which are most frequently mixed with land shells and fresh-water shells, which renders it probable that this wood grew in the same place where it is at present buried; and it agrees with all the other facts, showing that the surface of the globe was dry, and peopled with animals and vegetables, before the last irruption of the sea.

Two young and skilful naturalists, MM. Desmarets and Lemon, have found in the fresh-water beds in our neighbourhood shells of those small *entomostraceas* which have been named *cypris*, and likewise grains of the genus of plants known by the name of *chara*. Before them these grains were considered as shells to which the name of *gyrogonites* was given.

The geological system of the environs of Paris, which constituted the principal object of the observations and discoveries of MM. Brogniart and Cuvier, is now studied with great attention by many skilful naturalists. MM. de Tristan and Bigot de Morogue have described with care the parts of it which border on the Loire; and M. Omalius de Halloy, Engineer of Mines, while aiding them in their researches, and in those formerly made by our associate Desmarets, has employed himself in tracing out exactly all its limits, and in making a map of the whole. The beds of this system deposited upon chalk represent an irregular and curvilinear trapezium, the south side of which, parallel to the Loire, extends along that river south from Cosne to below Blois. On the east side it passes near the towns of Montargis, Nemours, Montereau, Ville-nose, Sezanne, Epernay, Laon, Crepy, La Fere; on the north side, near those of Chauny, Noyon, Compeigne, Clermont, Beaumont, Chaumont, and Gisors. Finally, on the west side, it descends by Mantes, Houdam, Epernon, Auneau, and along the Loire as far as Vendome, from which it goes to rejoin the Loire at Blois. All this space is surrounded with chalk, and chalk in which

M. de Halloy has recognized three very distinct modifications. It is itself surrounded, except towards the sea, by a compact limestone older than it, which forms a great part of Berry, of Burgundy, and of Lorraine as far as Vosges, and which appears beyond the Black Forest in Franconia and Hesse. The *formations* of the system of Paris extend over this chalk divers ramifications, and the agriculture, the industry, and all the resources of each place, are often determined by the geological constitution of the soil. M. de Halloy has not displayed less courage than sagacity in collecting the materials for his memoir ; for he traversed the whole country on foot, visiting the most inaccessible places when he could hope for any information, and being neither deterred by bad weather nor bad roads.

M. Brogniart, corresponding member of the Institute, has likewise visited a part of France no less interesting for geology, that which forms at present the department of La Manche ; and M. de Halloy, who visited it after him, has confirmed and completed his observations. From the description which M. Brogniart gives of the rocks of this country, and of their mutual position, it follows that what was considered as *granite* belongs to another species of rock, called *syenite* by Werner, and characterized by the hornblende which enters into its composition, as well as by its much more recent formation, than true granite. The syenite of the Manche reposes on slate, and other rocks posterior to granite. It appears even that in certain places it covers a lime-stone containing the remains of organized bodies ; a fact analogous to what Von Buch observed in Norway, and from which we may conclude that there have been precipitations of crystallized rocks after the manifestation of life in the waters which anciently enveloped the globe.

M. Brogniart, who is employed in drawing up a general treatise on geology, has presented the plan according to which he proposes to arrange the rocks ; that is to say, those aggregations of minerals which compose the crust of the globe such as we know it. Applying the principles at present acknowledged by all naturalists, he wishes the basis and the details of his method to repose upon characters taken from the rocks themselves, and he rejects all those taken from their mutual position on the globe, which belongs to their natural history, but not to their systematic arrangement. He separates from the rocks, and unites to simple minerals, the mineral bodies which appear simple to the naked eye, and the heterogeneous nature of which appear only by washing and other operations, which, though they cannot be called chemical analyses, yet alter the appearance and the tissue of these matters. Such are slate, clay, &c. The rocks thus reduced, or the *mixed rocks*, as M. Brogniart calls them, are subdivided into *crystallized* and *aggregated*. The first have their parts either nearly in equal proportions, or one of the constituents predominates over the others. In the first case the genera are established according to the essential constituents ; that is, those which are constantly found. In the second

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case they are determined from the base, or the predominating substance; and in both cases the number of constituents, and the structure resulting from their mode of union, serve to distinguish the species. The aggregated rocks are divided according as the cement which unites them is more or less apparent, and according to the nature of that cement, and the grains which occur imbedded in it.

In this undertaking, so important to serve as a base to the history, properly so called of rocks, the author has almost every where preserved the names given to them by M. Haüy in the arrangement of them which he has made in the Museum of Natural History.

M. Brogniart has likewise stated to the Class the division which he considers it as necessary to establish among rocks, considered with respect to the epoch of their formation, and to the remains of organized bodies which they contain, and which are the best marked characteristics of these epochs. Below all the rest occur the granitic formations destitute of all organic remains, the most ancient that we know. The beds that cover them contain only a very few remains of organized beings, and almost all belonging to the class of zoophytes. A third series, that of the syenitic beds, contains no traces of them; as if their formation had been for a time interrupted. In the fourth series shells begin to appear, especially those which have been called cornua ammonis, and which belong to the family of dry shells. The fifth and six series of beds are characterized by the gryphites and cerites, which predominate among their shells. Finally, there are beds in which the distribution is so irregular that they cannot be classed according to the order of time. These are the trap rocks on the one side, and on the other those produced by the ejections of volcanoes. In all these groups are mixed transported beds (*terreins de transport*) produced by the violent movements occasioned by successive revolutions, and indicating accurately the time when each commenced.

VEGETABLE PHYSIOLOGY AND BOTANY.

The fall of leaves in autumn, a phenomenon so well known, has occasioned some discussions respecting its cause, and several observations have been made respecting the variations in it. M. Carnot, member of the mechanical section of the Institute, but who does not neglect any thing that can furnish a subject for meditation, having remarked that some trees begin to lose their leaves at the top of their branches, and others at the bottom, M. Palisot de Beauvois, member of the section of botany, has endeavoured to discover the reason of this difference. He has found that in general those species, whose autumnal shoots consist of prolongations of the extremities of the branches, begin to lose their leaves at the under parts, while those whose shoots consist in small lateral branches begin to lose their leaves at the summits; or in other terms, that the last leaves which expand are likewise the last that fall off, Duhamel, who had made a similar remark, was surprised that those

leaves which ought to be the most delicate were the most capable of resisting the action of the frost; but it is not the frost which necessarily occasions the fall of the leaf. That fall is a necessary effect of vegetation; and whether by the developement of the bud, or by some other internal alteration prepared by nature, the footstalk separates when the progress of its nutrition has brought it to the point at which the tissue which served it as a connection is destroyed. Accordingly when a tree dies from any cause during the season of vegetation, the leaves continue to adhere.

· It is well known that various flowers open and shut at determinate hours, and that heat and humidity have a great influence upon this phenomenon. M. Desvaux, a botanist of Paris, has made observations on this subject on the *mesembrianthemums,* plants in which these alternate motions are so remarkable, that their generic name has been derived from it. He has found that the cause does not reside in the corolla, as had been supposed, but in the calix, which, in shutting, forces the corolla to obey its contractions; so that if the calix be removed, the corolla continues expanded by night as well as by day.

M. de Mirbel, our associate, has presented us this year with two series of observations. The first on the seed, and on the membranes which cover it; the second on the *pericarpium,* or the receptacle in which the seed is lodged. He has in the first place examined how far the analogy pointed out by Malpighi between the membranes which cover the fœtus of animals in the matrix, and those which cover the seeds of plants, is exact. The embrio consisting of the plumula and radicle being considered as the fœtus, Malpighi considered the *testa,* or outer covering, to represent the *chorion*; and the *tegmen,* or interior covering, to represent the *amnios.* The *perisperme* appeared to him to represent the liquid contained in the amnios, in which the fœtus swims. M. de Mirbel finds, on the contrary, that at first the grain is merely a mucilaginous and continuous cellular texture, one part of which becomes the embrio, and the remainder the perisperm and seminal tunics, without its being possible to say that at any period the embrio swims in a liquid. The mucilaginous state of this tissue, and its transparence, seem to have given occasion to the inaccurate comparison of Malpighi.

M. de Mirbel, passing to the examination of the pericarpium, has succeeded in reducing its form under a general law, which, determining what is essential in that part of vegetables, reduces almost to nothing the anomalies which it appears to exhibit in certain families. The general type of every pericarpium may in his opinion be represented by a little box flattened at the sides and composed of two valves, the union of which forms two edges or two sutors, one more curved and the other more straight. To this last sutor adhere the small seeds, and through it pass the vessels which go to the seeds, either from the body of the plant, or from the style or the organ which transmits the fructifying energy. This

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disposition is obvious in the pods of leguminous plants, such as kidney beans, peas, &c. We perceive it likewise very well in the kernels of almonds, peaches, cherries, &c. in which one of the sides has always a channel, and sometimes a canal, which points out the passage of the vessels. M. de Mirbel gives this simple capsule the name of *camare*. The plants which we have mentioned have only a single flower. When there are several, their semi-niferous or vascular sutors are always on the side of the ideal axis of the fruit; and if we represent them as united together, they form a single pericarpium box, divided into several compartments, and bearing the seeds along the central axis.

It is thus that in the same family the *camaras* are sometimes distinct, sometimes united, according to the genera, as we see in ranunculaceous, rutaceous plants. In like manner certain camaras, united at first, separate when they come to maturity, as in the rose *tremiere*, the *euphorbia*, the *hura crepitans*, &c.

This idea being once admitted, we find that pericarpiums very different at first sight are in fact only very slight modifications of a common design; but as it happens that families far distant from one another have usually pericarpiums very similar, we cannot often draw from that part of vegetables characters proper for the distribution of plants.

This is not the case with the internal structure of seeds, which are exceedingly different from one another in different families, and very little in the same family. This circumstance has partly led M. de Mirbel to divide the family of oranges of Jussieu into four families; namely, *aurantiacea*, already very well described by M. Cornea; *olacinea*, comprehending the *olax, fissilia, heisteria, ximenia*; *theacea*, containing the tea plant and *camelia*; and the *ternstromia*, comprehending *ternstromia* and *fresiera*.

In the family of alacinea is not included the *ximenia ægyptiaca*, of which M. Delille has properly made a new genus under the name of *balanites*. This plant, which we do not know at present where to class, has presented to M. de Mirbel a character which is perhaps unique in the vegetable kingdom. Botanists are acquainted with the glandular body placed under the pistils in a great number of flowers, to which the name of disc or nectarium has been given. It exists in the balanites under the form of a purse with shoots (*une bourse a jetons*). The pistil is at first entirely covered with it, and is not visible; but as it increases in bulk it separates the sides of the purse, and makes its appearance.

Article XIII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Dr. Spurzheim commenced his first Course of Lectures on the

Physiognomical System of the Brain, on Wednesday, the 19th inst. at No. 11, Rathbone-place. The Course consists of twelve lectures, which are given three times a week, at eight o'clock in the evening. As the Doctor has announced his intention to publish a work comprising the substance of his lectures, we propose to defer our observations on the subject till we review his volume.

The following arrangements have been made for Lectures at the Surrey Institution, in the ensuing season :—

Mr. Weeler, on Chemistry ; to commence on Tuesday, Nov. 15, and to be continued on each succeeding Tuesday.

Mr. Rippingham, on Eloquence ; to commence on Friday, Nov. 18, and to be continued on each succeeding Friday.

Mr. J. Mason Good, on Classical and Polite Literature; to commence on Friday, Jan. 6, 1815, and to be continued on each succeeding Friday.

Dr. Crotch, on Music ; to commence early in February.

II. *Armenian Literature.*

The Armenians have been long distinguished from the other nations of Asia, by their roving spirit, and keen application to trade. They have spread themselves over all the east, and in more than one particular exhibit a remarkable resemblance to the Jews. Attempts of considerable importance have been lately made by different enlightened Armenians to introduce the literature and sciences of Europe into their country. We presume the reader will not be displeased to find a short account of these proceedings in this place. The introduction of knowledge into Armenia, if we consider its position, would soon alter the face of Asia.

About the beginning of the 18th century, P. Mitaker, of Sebastos, formed the resolution of devoting his life to the introduction of knowledge into his country. Quitting the Pariardes mountains, he settled with a great many disciples at Venice, at the extremity of the Adriatic Gulf. His prudence and capacity overcame every obstacle, and he got a great many books printed in the Armenian language, and distributed through his country. Knowing the utility of a literary society to excite a taste for study, he formed an Armenian Academy at Venice.

Since the creation of this society, and the publication of numerous works which have originated from it, different kinds of studies have been cultivated in Armenia. The ancient *Haikien* (*Armenian*) literature, which had been dead for ages, assumed a new life under the care of the members of the Academy.

But it was at the commencement of the nineteenth century, when the respectable family called *Duz* declared itself its protector, that the society received a new organization more adapted to encourage the advancement of knowledge. This family has been long conspicuous in Armenia for its high rank, and its numerous virtues. John Duz, one of them, established a new society in Constantinople, under the name of *Arscharunian*; he provided the funds necessary for its support, and confided the regulation of it to two

persons of his family. He died on the 21st of April, 1812. His eldest son, Gregory, ordered the same labours to continue, both on account of the wishes of his father, his own love of science, and his patriotism. Though his time was devoted to the service of Government, he did not neglect the organization of the *Arscharunian* Society. In consequence of his instructions, James Duz,* his brother, and Baptiste Duz, his nephew, have just finished the most material part of their labours. Circular letters have been sent to the Armenians in every country. Every thing has been arranged; and measures have been taken to enable every Armenian for the future to co-operate for the general good, either by his talents or fortune.

Their principal object, like that of the illustrious Mikitar, the founder of the Academy, is to publish instructive books of all kinds, to print the ancient Armenian authors, especially the manuscripts, as they can procure them, to translate into Armenian the classical works both ancient and modern, to collect new information respecting Armenia, in order to communicate it to their countrymen, and to the philosophers of Europe. Botany, mineralogy, zoology, geography, must be improved by the labours of these learned men. Curious details have been published respecting the fish lately observed in the Euphrates and Araxes, respecting wild races of sheep and goats, and the splendid race of horses called *Nejouk,* of which so little is known in Europe.

III. *Explosion in a Coal-pit near Newcastle.*†

On Tuesday, Sept. the 6th, the foul air in the Leefield pit, belonging to Mr. Humble, in the parish of Chester-le-Street, fired at a hole, where a man was boring to ascertain the distance the men were off the waste, just at the moment that the men and boys descended to their work. Many of the men got out, when the viewers and several men descended again, and succeeded in bringing the remainder to bank, where they were laid to all appearance dead, but we are happy to say they all recovered that day. On Friday, as the men were descending again to promote the ventilation of the pit, it unfortunately fired again, when four men were killed, and five others much hurt. Some men from the neighbouring collieries descended on Saturday, and brought the sufferers to bank. Two of the deceased have left families, one a wife now pregnant and seven children, the other a wife and five children.

* James Duz was partly educated in Paris. The French language, music, painting, natural philosophy, chemistry, and mineralogy, occupied him exclusively while he resided in the French capital. Since his return to his country he has continued his studies with ardour. His example excited the emulation of Baptist Duz, who chose to be his fellow labourer. These two are well acquainted with French, English, and other European languages; and much benefit must result to Armenia from their enlightened exertions.

† This explosion was mentioned in the last number of the *Annals.* The notice is copied from the Newcastle Chronicle, of Sept. 17.

IV. *New Volcanic Island.*

A very singular phenomenon has occurred in the Russian province of Tschernemerk, in the vicinity of Alteruzruk, immediately opposite the salt-works. On the 10th of May, at two *p. m.* the weather being calm and serene, a noise was suddenly heard from the sea, about 200 toises from the shore. Flames issued forth, accompanied with explosions resembling those of cannon; thick clouds of vapour rose at the same time; and enormous masses of earth and huge stones were hurled with force into the air. Ten very violent eruptions took place successively within a quarter of an hour. Those that followed were protracted till late in the evening. Then an islet was seen to rise from the sea, pouring forth from several mouths a bituminous matter, which subsequently assumed a firmer consistency. While this phenomenon was operating, a particular smell spread about the distance of ten wersts around : this smell had not any resemblance to that of sulphur. On the 20th the examination of this islet was commenced ; it was thought to be inaccessible, being surrounded on every side by hardened bitumen ; at last they succeeded in penetrating to the interior ; its elevation is a toise and a half above the level of the sea, and its surface entirely covered with a stony whitish mass.

V. *Sulphate of Strontian.*

Sulphate of strontian has been lately discovered at Süntel, in Hanover, by Professor Hausmann, who has published a particular description, together with a chemical analysis of it by Professor Stromeyer. As it is the first example of the occurrence of this mineral in Germany, it has excited the attention of the mineralogists of that country. It constitutes a bed about one fourth of a fathom thick in a coal-mine, and seems to be connected with the shell limestone in the north of Germany. It lies above a bed of greyish coloured floetz limestone, and is covered by a very thick bed of clay marl. Its colour is partly white and partly grey. It occurs both amorphous and crystallized. Hausmann discovered in it the various kinds of crystals described by Haüy and two new species, namely, rectangular, four-sided tables levelled on two or on all the edges. Their symbols, according to Haüy's method, are $\overset{2}{A}\overset{1}{E}P$ and $^{1}H\ i\ \overset{1}{E}\ P$. The specific gravity is between 3·8190 3·9064. According to the analysis of Stromeyer, this mineral is composed as follows :—

Sulphate of strontian	97·208
Sulphate of barytes	2·222
Silica	0·254
Black oxide of iron	0·116
Water and a little petroleum	0·190
	99·990

He could not detect any lime in it; at least the earthy carbonates obtained from it and dissolved in nitric acid did not yield any soluble matter when digested in alcohol.

Still more lately, mine-master Ribbentrop observed a rock not far from Karlshütte on the road between Gottingen and Hanover, which on examination proved to be sulphate of strontian. According to the analysis of Stromeyer, its constituents are,

Sulphate of strontian ....97·601
Sulphate of barytes .... 0·975
Silica............................. 0·107
Oxide of iron 0 646
Water or loss by a red heat .... 0·248
                                      ——————
                                      99·577

According to Hausmann, this rock constitutes a portion of the shell limestone formation. It is quite similar in its geognostic relations to the sulphate of strontian of Süntel.

VI. *Salts of Strontian.*

Joseph Moretti, Professor of Chemistry in Mailand, has published some facts respecting the salts of strontian. British chemists will probably like to be acquainted with these facts. I shall therefore state here such of them as are not already generally known.

1. When succinic acid or arsenious acid is dropped into barytes water, a precipitate falls; but no precipitate is produced when these acids are dropped into strontian water. Hence the two liquids may be easily distinguished by means of these acids.

2. Six or eight parts of boiling sulphuric acid dissolves one part of sulphate of barytes or of sulphate of strontian. The strontian solution remains clear and unaltered when the liquid is cold, and does not let the salt fall unless it be diluted with water; but the barytes solution crystallizes, on cooling, in needle-form groups. Here then is a method of distinguishing the two salts from each other.

3. Very little arsenic acid is required to neutralize a considerable portion of strontian. This acid accordingly has a stronger affinity for strontian than any other. When sulphate of strontian and arsenic acid are boiled together, arseniate of strontian is formed, and sulphuric acid disengaged. When arsenic acid is dropped into a solution of sulphate of strontian, arseniate of strontian immediately precipitates. Arsenic acid acts in the same manner on sulphate of barytes.

4. Arsenious acid forms with strontian a soluble and uncrystallizable salt. When arsenite of strontian is dropped into barytes water, arsenite of barytes precipitates in flocks. Hence arsenious acid has a stronger affinity for barytes than for strontian.

5. Superoxalate of strontian is soluble in water.

6. Succinic acid occasions no precipitate in strontian water.

Hence it follows that succinate of strontian is soluble in water.
When the solution is concentrated by evaporating, small crystals of
succinate of strontian appear; but they disappear when the liquid
cools.　When the liquid is evaporated to a thick consistency, the
succinate of strontian falls in the form of a powder.　This salt has
a peculiar taste, not unlike that of muriate of potash.　It is equally
soluble in cold and in hot water.　When succinate of strontian is
dropped into barytes water, succinate of barytes immediately pre-
cipitates, showing that barytes has a stronger affinity than strontian
for succinic acid.

VII. *Dr. Olinthus Gregory.*

In the last number of the Philosophical Magazine this Gentle-
man has thought proper to insert a most virulent attack upon the
Editor of the *Annals of Philosophy*, because he refused to admit
into this Journal a continuation of his abuse of the Royal Society.
The Editor of the *Annals of Philosophy* considers himself as pledged
to the public not to fill his pages with matter totally unworthy of
the attention of his readers; and on that score Dr. Gregory must
excuse him for refusing to circulate low abuse against one of the
most celebrated and meritorious societies in Europe.　Dr. Gregory,
in his last paper, has totally altered the subject on which I animad-
verted.　He had affirmed, or at least insinuated pretty plainly, that
the object of the Royal Society in publishing Don Rodriguez'
paper was to sink British astronomers and exalt those of France.　I
merely said, in answer to this, that the Royal Society had no such
object in view; that it was very possible that an error might have
been committed at Arbury Hill, and that the suspicion could be
removed only by repeating the observations.　Dr. Gregory under-
took to demonstrate that it is *impossible* that an error of $4\frac{1}{2}''$ could
have been committed at Arbury Hill; and (forgetting his former
zeal for British observers, and his philippic against the Royal Society
and the French,) he has appealed to Delambre, a French astro-
nomer, who he says is the greatest astronomer now living; and
Delambre thinks it unlikely that there should have been such an
error committed at Arbury Hill.　Therefore, &c.　Q. E. D.

Now I must acknowledge that I do not perceive any demonstra-
tion here.　This indeed may be owing to ignorance on my part; for
we have Dr. Gregory's authority for saying that I am totally unac-
quainted with the subject, that I am no philosopher, and a man of
very moderate knowledge and abilities.

I am well aware that this is not the first, nor the second time, in
which Dr. Gregory has indulged himself in this kind of language
with respect to myself; nor am I ignorant of the dishonourable
motives which induced him to compose the reviews to which I
allude; and the cool malignity with which he volunteered to blast
the character and injure the reputation of a man who never had
done him any injury, with whom he was not personally acquainted,
and who, having attached himself to a branch of science different

from that which he himself cultivated, could not even be an objec
of jealousy. If this be the character, and these the occupations o
philosophers, I am obliged to Dr. Gregory for declaring that in hi
opinion my name does not deserve to be placed in the infernal list.

He complains in his paper that I write in a dictatorial style, and
have assumed an air of superiority which but ill accords with the
palpable mediocrity of my talents and knowledge. I am not
conscious to myself of assuming any such superiority; but I consider
myself as entitled, as well as others, to give my opinion upon every
subject on which I write. I think it my duty, as the editor of a
scientific journal, to state my sentiments respecting the various
topics discussed in it. Many of my readers, I know, are gratified
by this line of conduct; for I have received various letters on the
subject, some of them requesting me to go rather farther than I
consider as consistent with the rules of decorum. If Dr. Gregory
does not choose to adopt my sentiments, he is at liberty to reject
them; but he must not expect that I am to enter into a controversy
respecting every opinion which I happen to give in my Journal.
The consequence of this would be that my whole time would soon
be taken up in disputing on subjects with most of which I have not
the least concern.

Pray, in the name of common sense, what has Dr. Olinthus
Gregory done to entitle him to arrogate to himself the names of
mathematician, philosopher, man of knowledge and of talents?
Has he made any addition to any branch of science whatever?
Except his demonstration that an observer cannot possibly commit
an error of $4\frac{1}{2}''$ in determining the position of fixed stars, which I
agree with him cannot be paralleled in the writings of Archimedes
or Newton, I myself never heard of any of his investigations or dis-
coveries; though I have looked over, I believe, most of the books to
which he has attached his name.

VIII. *Transition Granite.*

There seems to exist at present in Germany a great desire to dis-
cover transition granite. If they go on a few years at the present
rate the whole granite in existence will probably become transition.
Raumer has published a paper on a portion of the Saxon Erzge-
birge, in which he maintains that it contains granite, gneiss, mica-
slate, porphyry, and syenite, all resting on grey-wacke. Von Buch
informs us that great tracts of grey-wacke country in Switzerland
contain abundance of gneiss. It is also said that the granite of the
Hartz, and of some districts of Thuringia, is transition. We must
have a little patience before we adopt these sweeping conclusions.

IX. *Mineralogy of the North-East of Scotland.*

Professor Jameson, of Edinburgh, has lately made a mineralo-
gical tour along the north-east coast of Scotland. I shall take the
liberty of stating from his last letter some of the observations which
he made. In Kincardineshire there are beds of trap and porphyry
alternating with red sand-stone, just as in Arran, the Lothians,

Argyleshire, &c. The prevailing rock round Aberdeen is granite, not syenite; and on the coast immediately beside Aberdeen are fine displays of granite veins in the gneiss. These veins are extremely interesting, as they exhibit on the great scale appearances which in other quarters are usually very diminutive in point of size. At Peterhead the rock is distinct syenite. Kinnaird's Head exhibits fine examples of granite veins and beds. The Braes of Aberdour, and the stupendous headland called Tronk Head, are of sand-stone and conglomerate. The coast at Macduff exhibits fine cliffs of grey-wacke, grey-wacke-slate, &c. Around Portsoy there are fine examples of serpentine in beds, and enormous beds of quartz, at times assuming the magnitude of mountain masses. In this quartz, which is decidedly primitive, there are imbedded masses very much resembling the vegetable casts found in the sand-stone of the coal formation. This singular fact induces Mr. Jameson to hesitate about admitting these supposed petrifactions to be the real moulds of vegetables.

X. *Wood Tin.*

Mr. Mawe, of the Strand, London, lately received from Mexico a number of magnificent specimens of wood tin. It would appear that they occur in that country stalactitical in mammelated chalcedony. One of these specimens weighed $10\frac{1}{2}$ ounces, was of a beautiful form, and beyond comparison the finest specimen of that substance that has hitherto occurred.

XI. *Goniometer.*

It will probably be interesting to crystallographers to hear that Mr. Schmalcalder, philosophical instrument-maker in London, has lately made a very material improvement on the common French goniometer, which makes it much better adapted for measuring the angles of crystals. The semicircle is entirely separated during the measurement, and afterwards applied merely to determine the angle. Those who have been accustomed to employ the French goniometer will perceive immediately how much this improvement facilitates the use of the instrument.

XII. *White-stone.*

This is the name of a rock which Werner has included among the primitive formations. It has been generally considered as a variety of felspar; but from a late analysis of Klaproth this opinion does not appear to be correct. He found its constituents as follows:

Silica	80
Alumina	12
Potash	5
Oxide of iron	1·5
Water	0·5
Loss	1
	100

ARTICLE XIV.

METEOROLOGICAL TABLE.

———

1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
9th Mo.										
Sept. 7	S W	29·94	29·67	29·805	62	50	56·0	—	·37	☾
8	N W	30·05	29·94	29·995	65	50	57·5	—		
9	N	30·06	30·05	30·055	64	49	56·5	—	—	
10	N	30·07	30·06	30·065	62	47	54·5	—	—	
11	N	30·15	30·06	30·105	65	38	51·5	—		
12	N E	30·15	30·13	30·140	62	33	47·5	—		
13	E	30·14	30·10	30·120	63	34	48·5	·37		●
14	E	30·14	30·12	30·130	65	34	49·5	—		
15	E	30·12	30·05	30·085	64	36	50·0	—		
16	E	30·00	29·98	29·990	66	43	54·5	—		
17	E	30·01	29·95	29·980	68	37	52·5	—		
18	S E	30·05	30·01	30·030	71	39	55·0	—		
19	S E	30·02	29·97	29·995	72	44	58·0	—		
20	S W	29·97	29·76	29·865	75	52	63·5	—	—	
21	S W	29·76	29·70	29·730	68	44	56·0	·56	·16	☾
22	S W	29·79	29·75	29·770	65	40	52·5	—	1	
23	S	29·75	29·59	29·670	65	48	56·5	—	—	
24	S	29·57	29·52	29·545	70	60	65·0	—	—	
25	S W	29·77	29·74	29·755	70	44	57·0	—	·35	
26	S E	29·74	29·65	29·695	66	55	60·5	—	—	
27	W	29·76	29·75	29·755	66	48	57·0	—	·31	
28	N	29·97	29·76	29·865	64	44	54·0	·25	—	
29	N	29·97	29·94	29·955	61	54	57·5	—	—	○
30	N E	30·00	29·94	29·970	64	46	55·0	—		
10th Mo.										
Oct. 1	N E	30·06	30·00	30·030	54	40	47·0	—		
2	N E	30·16	30·06	30·110	54	37	45·5	—		
3	N E	30·20	30·16	30·180	57	37	47·0	—		
4	N E	30·20	30·03	30·115	60	39	49·5	—		
5	N E	30·03	29·79	29·910	59	31	45·0	·43		
		30·20	29·52	29·945	75	31	53·79	1·61	1·20	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Ninth Month.—7. Showery. Wind veered to N. W. In the evening *Nimbi*, with large *Cumuli*: rain in the night. 8. Overcast, windy: calm evening. 9. Cloudy: a few drops. 10. Windy, with *Cumulus* and *Cirrostratus*: a slight shower at eight *p. m.* 11. Windy: fair day: *Cumulus*, with *Cirrostratus*, densely grouped at sun-set. 12. Much dew: orange sky at sun-set. From 11 to 15 inclusive, hoar frost in the mornings, and the *Stratus* by night. 16. Brisk wind: calm at night. 17. A very wet mist this morning: the day was fine, with large *Cirri*. 18. Fine: a group of *Cirri* obscured the setting sun. 19. Misty morning: clear day. 20. *Cirrus* in streaks from N. to S., mixed with haze lowering and passing to *Cirrocumulus* and *Cirrostratus*, the motion being from E. to W. A shower of rain followed these appearances. 21. Brisk wind: changeable sky. 22. Dew this morning: the sky overcast with *Cirrostratus* and haze: *p.m.* windy, with a shower. 23. *a. m.* Misty, with *Cirrostratus*. Showers, with wind, followed. 24. *a. m.* Blustering at south: temp. at the maximum at nine. Cloudy, damp, and close. In the evening pretty much lightning to the S. and W.: at nine the storm came hastily over us, giving a few discharges of blue lightning, with thunder and heavy rain. 25. A small steady rain, succeeded by calm sunshine. 26. *a. m.* Dew: afterwards cloudy and windy, with showers. 27. Cloudy, *a. m.* A *Stratus* at night. 28. Overcast: a slight shower, succeeded by *Stratus*. 29. Windy: overcast: a few drops. 30. Windy.

Tenth Month.—1 to 5 inclusive, clear weather, with brisk drying winds.

RESULTS.

Winds variable: the south-west uniformly brought rain; the opposite current, fair weather.

Barometer : Greatest height..........30·20 inches;
　　　　　　 Least.................29·52
　　　　　　 Mean of the period ......29·945

Thermometer: Greatest height..........75°
　　　　　　　 Least.................31
　　　　　　　 Mean of the period ......53·79

Evaporation, 1·61 inch.　　　　　Rain, 1·20 inch.

TOTTENHAM,　　　　　　　　　　L. HOWARD.
Tenth Month, 12, 1814.

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1814.

ARTICLE I.

Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

THE best experiments on organic substances are certainly those which Gay-Lussac and Thenard have published in their Recherches Physico-Chimiques, ii. 265. These chemists burnt various ternary and quaternary oxides with hyper-oxymuriate of potash, moistened with water, and made up with it into small balls. These balls were dried at the temperature of boiling water, and were afterwards burnt in an apparatus contrived on purpose. They collected over mercury the gaseous products of the combustion, and determined exactly the relative volumes of carbonic acid gas and oxygen gas. The quantity of oxygen furnished by the hyper-oxymuriate being known, and added to the weight of the substance decomposed, they found what the products of the combustion ought to weigh. By finding the weight of the carbonic acid and oxygen evolved, and subtracting it from the original weight, they obtained the quantity of water formed. The quantity of carbonic acid and water thus found indicated the quantities of carbon and hydrogen in the substance analysed, while the superfluous portion of oxygen above that yielded by the salt was obviously likewise a constituent of the substance. We shall find afterwards that several of the analyses of these chemists agree with my own.

I trust I shall be permitted here to make a few observations upon the Work of these chemists, so justly celebrated, in order to explain my reasons for not following their analytical method, and of only

having borrowed from them the use of the hyper-oxymuriate of potash, which I substituted for the brown oxide of lead that I was in the habit of employing before I was acquainted with the experiments of the French chemists.

1. The apparatus of Thenard and Gay-Lussac has a stop-cock, through the opening of which the balls must pass, in order to be received into a metallic tube, the lower extremity of which is to be heated red-hot. The stop-cock requires to be well greased in order to answer the purpose. Now as the balls are obliged to make half a turn in this stop-cock before they fall down, it is scarcely possible to prevent them from taking up some grease, which will be decomposed along with them, and render the result to a certain degree inaccurate.

2. The necessity of moistening the substance under examination in order to form it into balls with the hyper-oxymuriate of potash, prevents the possibility of reducing it to the same degree of absolute dryness as before the operation, and may sometimes occasion even an alteration in the mass during the drying. It is only by some such circumstance that I am able to explain the differences which sometimes exist between the result of the analysis of the French chemists and mine.

3. But the most important objection is, that in their method the quantity of hydrogen is determined by the loss which in some cases may be owing to other unforeseen circumstances, and which in all cases ought to be a little more than the quantity of water produced. Now we shall see hereafter that it is a very essential point to be able to determine with the most rigorous exactness the quantity of hydrogen in these substances, because, as its volume is very light, a small error in the experiment may occasion several volumes too much of hydrogen ; while it would require a very considerable error to occasion a mistake in the number of volumes of oxygen or carbon.

4. Another observation respecting the experiment of the French chemists, which however does not affect their method, is that Gay-Lussac and Thenard paid no attention to the water of combination in several organic bodies. They satisfied themselves with drying them at the temperature of boiling water, and did not examine whether the substances which they considered as dry contained water or not. This circumstance is by no means indifferent, as we shall see hereafter. They made some of their analyses on vegetable acids combined with lime and barytes, without attending to the water contained in these salts. Thus considering the mixture of acid and water as pure acid, their result differs of course very considerably from the truth. When we correct it, by subtracting this quantity of water, it agrees in general with mine.

Thenard and Gay-Lussac paid no attention to chemical proportions. This is not suprising ; because, when they wrote, these proportions were unknown, though we possessed very good indications of them. Chemistry is indebted to the genius of Gay-Lussac for

one of the principal of these ; I mean the discovery of the relation between the volumes of gasiform bodies that combine together.

I shall now state what I have done myself in this kind of analysis. In the year 1807, during my first experiments on determinate proportions in chemistry, which were suggested to me by the excellent experiments of Richter (Über die neuen gegenstände der Chimie, st. ix. and x.), at a period when I did not foresee the extent of the subject, I formed the resolution to analyse the vegetable acids with more care than my predecessors, beginning with acetic acid, whose capacity of saturation I had already ascertained. I attempted this analysis by distilling a mixture of effloresced acetate of lime and minium. I obtained in different experiments quantities of water nearly equal ; but the carbonic acid, which was absorbed by barytes water, varied considerably, on account of the production of carbonic oxide, which I could not prevent. This led me to abandon the experiment.

But after I had examined a great number of inorganic bodies, I thought it requisite to return to organic substances, in order to determine how far the laws which I had ascertained in inorganic nature would apply also to organic bodies.

I analysed at that time oxalic and tartaric acids, and I gave an account of the results which I obtained in Gilbert's 'Annalen der Physik, February, 1812 ; and in the Annales de Chimie, May, June, &c. 1812. To perform these analyses I mixed the oxalate or tartrate of lead with the brown oxide of lead, and distilled the mixture, making the gases evolved pass through a tube filled with muriate of lime. The carbonic acid gas was absorbed by limewater ; so that the carbonate of lime formed could be collected and weighed. By this method I obtained the water and carbonic acid with sufficient precision to enable me to calculate the hydrogen and carbon in the acids. The loss was oxygen; and if this was a multiple by a whole number of that in the oxide of lead combined with the acid, I considered my analysis as correct.

But this method of analysis had inconveniences which rendered it less exact. The decomposition took place with too much rapidity, so that it was difficult to keep the joinings air-tight; and it always gives a little more carbon and hydrogen than the substance analysed contained, which rendered the proportion of oxygen too small, as may be seen by the results of these analyses laid before the public. I found a method of correcting the first of these inconveniences, by putting the mixture to be burnt in a tube, and heating only a part of this tube at a time, commencing at the open extremity, and proceeding gradually to the shut end. But I found it impossible to correct the errors in the result, because the red lead from which I prepared my brown oxide contained always some impurities, which I could not remove, and which remained mixed with the brown oxide ; so that this oxide, when decomposed alone by heat, gave always traces of moisture and carbonic acid gas.

In consequence of this I put off again the experiments on

organic bodies, in hopes of finding hereafter a suitable method. Soon after the experiments of the French chemists came into my possession, they suggested the idea of substituting hyper-oxymuriate of potash for the brown oxide of lead. This method was attended with success. I shall pass over my numerous unsuccessful trials which led me by degrees to the method that I shall now describe.

The substance to be examined must be obtained in a state of purity, and quite free from water. It is frequently best to employ it in a state of combination ; for example, with oxide of lead. It is mixed in a mortar with five or six times its weight of hyper-oxymuriate of potash, previously reduced to powder, and strongly heated. When perfectly mixed, nine or ten times their weight of common salt, which has been recently melted in a platinum crucible, is to be added, and the whole is to be well mixed. This mixture is introduced into a glass tube, having a diameter between a half and five-eighths of an inch, and sufficiently long to contain the whole mixture. This tube (fig. 1, plate XXV.) is shut at one of its extremities. A mixture of common salt with one or two grains of hyper-oxymuriate of potash is first put into the tube, and to fill about half an inch of its lower extremity. Then the mixture to be examined is introduced. I usually increase the proportion of muriate of soda in the last quarter of the mixture, to have it more in my power to moderate the combustion at its commencement. What remains attached to the mortar is removed by means of common salt in coarse powder. It is then put into the tube, and the whole covered with a mixture of common salt and hyper-oxymuriate of potash ; so that the mixture containing the combustible substance is included between two beds containing hyper-oxymuriate without any combustible matter. My reason for this is as follows : The first effect of the heat decomposes the hyper-oxymuriate in the anterior part of the tube, and fills the tube with oxygen gas ; so that the decomposition of the combustible matter commences in an atmosphere of oxygen. On the other hand, when the decomposition is finished, the tube and vessels contain a mixture of carbonic acid and oxygen gases. The last portion of the hyper-oxymuriate, the decomposition of which terminates the experiment, gives out oxygen, which forces the carbonic acid gas out of the vessels into the pneumatic trough, and this happens the more completely because the diameter of these vessels is too small to permit the gases to mix.

As to the shape of the tube represented in fig. 1, I always make it in the first place with a neck, through which I could introduce the mixture ; and when this is done, I draw it out, by means of a lamp, into the form represented in fig. 2. The intention of the length of the neck was to be able to give it the shape of fig. 2, without introducing into it any humidity produced by the flame of the lamp with which the glass is heated ; and in order not to be obliged to employ tubes unnecessarily long, I introduce the end of the tube, fig. 2, into a very small and thin glass represented of its

Plate XIV

Fig. 1

Fig. 2

Fig. 3

Fig. 4

Fig. 5

Fig. 6

Fig. 7
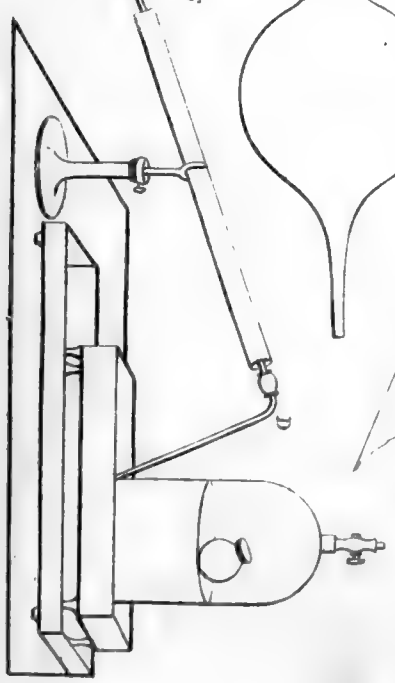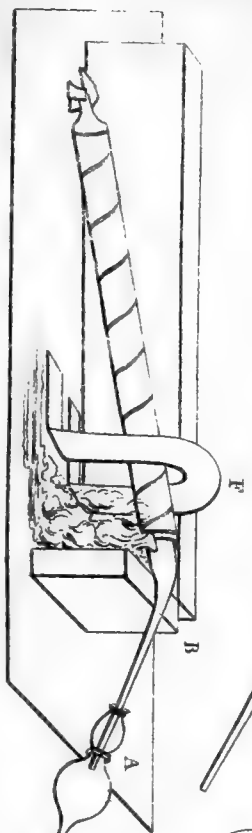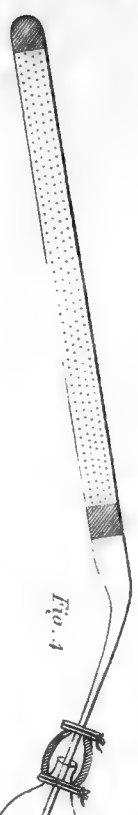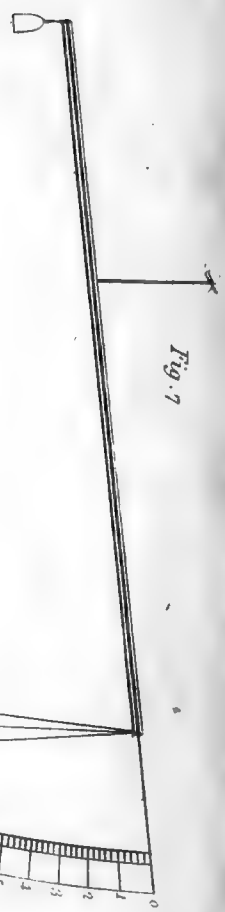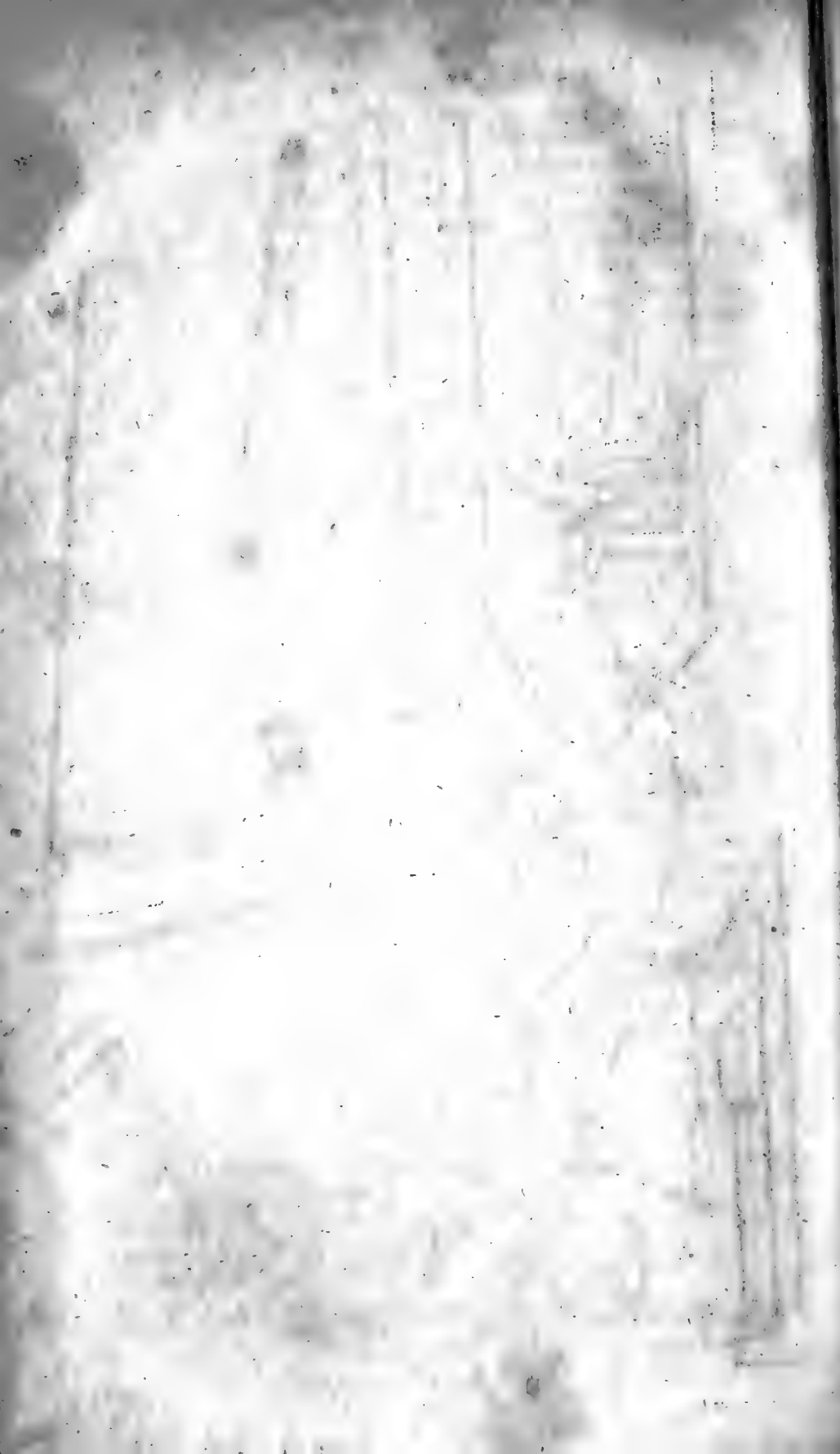
natural size in fig. 3. This glass, which I call the receiver, is joined to the tube by a caoutchouc tube* about an inch long, and of the requisite diameter. I tie the elastic tube with a silk thread both to the tube and to the receiver. By this contrivance I obtain a joining at once moveable and air tight. The elongated part of the tube penetrates through one quarter of the receiver. Fig. 4 shows how that is arranged. I attach the tubulure of the receiver, by means of another elastic tube, to a long glass tube filled with muriate of lime. This salt had been heated to redness, but not melted. Of course it preserved a spongy texture, presenting a great deal of surface, without rendering the tube which contains it too heavy. This tube is 20 inches long, and one-fourth of an inch in diameter. At its two ends it was shut by smaller tubes, the openings of which, that entered into the greater tube, were covered with muslin, to prevent the muriate of lime from falling out. These little tubes were firmly attached to the great tube by means of sealing-wax. One of these little tubes communicated with the receiver by means of an elastic tube, the other by means of a similar elastic tube with a curved glass tube, which conveyed the gases to the pneumatic tub. The weight of the small receiver with the two elastic tubes was about 85 grains troy, and that of the tube containing the muriate of lime about 772 grains.

To strengthen the glass tube that was to be exposed to the fire, I surrounded it with a piece of tin plate, very thin, and curved so as to form a tube. This I kept fast by means of an iron wire that was wrapped round it. I was obliged to have recourse to this precaution, because no glass tube could have resisted the pressure of the mercury in the pneumatic trough, after the glass began to soften in the fire.

The tube thus secured was placed between bricks, as may be seen in fig. 6 (in which however one of the two side bricks is not represented). Fire was kindled round the anterior extremity of the tube, taking care to secure the posterior parts from its influence by means of a screen, represented at F, fig. 6. This screen had a hole in it, through which the tube passed; and as the disengagement of gas began to slacken, I drew it by half an inch at a time towards the posterior end of the tube. By this means I was enabled to regulate the decomposition at pleasure. In most of my experiments I burnt from five to eight grains troy of the substance under examination mixed with from 30 to 40 grains of hyper-oxymuriate of potash. I took care to redden the anterior part of the tube before the heat began to act upon the mixture. The decomposition usually lasted from an hour and a half to two hours.

I received the gases formed over mercury in a glass jar capable of

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* I make these tubes in the following manner: I take a thin piece of caoutchouc, and heat it a little. I bend it, and cut off from the bending a small portion with a pair of scissars. The cut surfaces unite together, and form a tube. If they do not unite, let them be pressed with the nails against each other, taking care not to touch them with the fingers.

containing 33 cubic inches. The carbonic acid gas was absorbed by caustic potash in a small glass exactly weighed, and represented of its natural size in fig. 5. The increase of weight of this small glass indicated the quantity of carbonic acid.

The caustic potash employed in these experiments ought not to be heated red-hot beforehand, otherwise it will contain too little water for the formation of carbonate. It will soon become covered with a hard impenetrable crust, which will put an end to the absorption. We obtain it in a proper state when we evaporate it till it becomes hard on cooling. We cannot employ quick-lime, which absorbs scarcely any thing, while hydrate of lime lets go its water in proportion as it unites with the acid, and this water condenses in the glass jar. To determine exactly the weight of the small glass containing the potash, I cork it well, and weigh it cork and all. Then I take out the cork, and cover the mouth of the glass with a thin piece of glove leather, which I fasten round its neck by means of a silk thread. It is then immediately introduced into the mercurial jar. That I may be able to regulate the movements of this little glass, I tie to its bottom a small iron wire, by means of which I could withdraw it out of the jar, if that were necessary. The glass jar which I employed had a stop-cock at its upper extremity, to which I could attach a small air-pump. By this means I filled the jar with mercury.

When the decomposition was finished, I removed the tube, D, fig. 6, from the mercurial trough, and wiped away the mercury that might be attached to it. I then cut the neck of the tube remaining in the fire at B by means of a file. Lastly, I separated the receiver from the tube filled with muriate of lime. I now removed the silk threads at A and C, and weighed the receiver together with the piece of glass cut from the great tube. I then removed this piece of glass, dried it thoroughly, and weighed it by itself. By subtracting this weight from the increase of weight in the receiver I learned the quantity of water contained in the receiver. I then weighed the tube with the muriate of lime, which never gained in weight more than $0 \cdot 1$ grain, and often a great deal less. In this way I was sure of determining the whole of the water formed during the combustion; and if in some cases the experiments have given inaccurate results, we are neither to ascribe this to the apparatus nor the method, but to the substances introduced, which were not absolutely dry. The only circumstance connected with the method which can occasion any inaccuracy with respect to the quantity of hydrogen is neglecting to pulverize the mass in a hot mortar. Error may result also if the hands of the operator be in a state of perspiration, or if he allows his breath to play upon the mass which he is pulverizing. I have always taken care to keep the mortar as far off as possible, and to wear gloves during the whole arrangement of the experiment. We shall see by the analysis of oxalic acid how delicate this analytical method is, and how exactly it points out the most minute quantities of hydrogen. In that

analysis the water obtained from 690 grains did not exceed 0·3 grain in weight.

The small glass containing the potash remained usually 24 hours in the gas, though five or six hours are sufficient. I then allowed the mercury to fall by opening the stop-cock, and I removed the jar to prevent any accident in withdrawing the small glass through the mercury. I removed the leather very quickly, and corked up the glass with the same cork that had been formerly employed. I then wiped its exterior surface clean, and weighed it. In several cases I examined the residual oxygen gas by burning it along with hydrogen, but never could discover the smallest trace of carbonic oxide.

I must observe that in these experiments a small quantity of carbonic acid remains combined with the soda in the burnt mass. The oxide of lead decomposes the muriate of soda, and forms a submuriate of lead, setting at liberty a corresponding quantity of soda. The free alkali seizes upon the carbonic acid requisite for its saturation, and of course this quantity remains in the burnt mass. It would be difficult to determine by a direct experiment the quantity of carbonic acid retained by the soda, because the greatest part of this carbonic acid may remain dissolved in the water requisite for the solution of the salts. But we may easily calculate that quantity; for in the submuriate of lead the acid is combined with four times as much base as in the neutral muriate ; and consequently the oxide disengages a quantity of soda which requires, in order to become a subcarbonate, one-fourth as much carbonic acid as would be requisite to carbonate the whole oxide of lead present; that is to say, almost exactly one-twentieth of the weight of the oxide of lead. We must then in each experiment add this quantity of carbonic acid to what we obtain in the state of gas. But there is one exception, and that is in the analysis of oxalic acid, where a portion of the oxide of lead is always converted into peroxide, and this portion does not decompose the muriate of soda.

There are in general three methods of determining whether the experiment has given an accurate result or not. These are the following :—

1. The repetition of it with the same result. This proves that the experiment has been well executed. But it does not decide whether we have a method of operating capable of giving a correct solution of the problem. For this purpose we must

2. Examine if the loss, which represents the oxygen of the substance analysed, be a multiple by a whole number of the oxygen in the oxide of lead with which it was united. If this be the case, we estimate the number of volumes of oxygen in the substance analysed the same as the multiple which it is of the oxygen in the oxide of lead. Then we examine if the carbon and hydrogen constitute a certain number of entire volumes. In that case the experiment has every appearance of being exact. But still another check is requisite, without which we may still be deceived. We must

3. Examine if the substance can combine with the oxide of lead, or with some other binary oxide, in a greater proportion. In that case the number of times that the binary oxide is multiplied in the new combination points out whether the number of volumes of oxygen found be true or not; for example, if the number of volumes of oxygen in acetic acid be three, the subacetate ought to contain, combined with the same quantity of acid, either three or six times as much base as the neutral acetate, and not two or four times as much, as is obvious from the laws of definite proportions. Unfortunately this check can be applied only in a few cases. When it is wanting we must rest satisfied with the two preceding ones. Suppose, for example, that we have a vegetable salt to examine with a base of lead, and that this salt contains combined water, of which it cannot be deprived, and of the existence of which in consequence we cannot be absolutely sure; when we analyse it we find the elements of the water mixed with those of the acid; but we do not perceive this, because the hydrogen of the water constitutes a certain number of entire volumes, and because the oxygen is always a multiple of the oxygen of the base. In that case the third check proves the existence of water; but unless we can have recourse to it we are deceived by the analytical result, unless we can find an anhydrous combination of the acid examined, and expose it to an analytical experiment.

If by these reflections the reader perceives on one side how necessary it is to be circumspect in these experiments, and to seize every circumstance that can throw light upon them, in order to avoid being deceived by false resemblances, I hope he will see equally on the other side that by careful experiments and due circumspection we shall at last arrive at results respecting organic bodies almost mathematically exact, at least as far as the relations between their elements are concerned.

I shall now say a few words on the method of obtaining and analysing combinations of the ternary oxides with oxide of lead. We prepare a combination of the substance to be examined and pure ammonia, and there ought to be no excess of alkali. A neutral solution of nitrate of lead is poured into this solution drop by drop, taking care not to add an excess of it, for in that case the precipitate often contains traces of nitrate, which cannot be removed by the most careful washing. We obtain the subsalts by digesting this precipitate, well washed, and dried in caustic and concentrated ammonia. The washed precipitates are dried in the temperature of boiling water; or if exposure to the air be capable of altering their composition, we dry them in a vacuum, introducing along with them a quantity of sulphuric acid, having as large a surface as possible. To deprive these vegetable substances, or their combinations, of all water chemically combined or mechanically adhering, I employed the following method:—I introduced the dried substance into a flagon, the mouth of which I covered with paper. This flagon I plunged two-thirds into a sand-bath, which I had

prepared, of a large porcelain mortar filled with sand, which I had previously raised to the heat of 212° or 230°. I put this hot mortar under the receiver of an air-pump, along with a quantity of concentrated sulphuric acid, and immediately pumped the air out of the receiver. I do not believe that many vegetable substances could retain water under such circumstances ; at least I do not think that any compound of them with oxide of lead could do it.

We must never employ acetate or subacetate of lead to form insoluble compounds between organic bodies and oxide of lead, because a part of the acetate combines so intimately with the precipitate that it cannot be separated by washing. Hence if we employ such a combination for an analytical experiment, the result becomes very incorrect, without our being able to perceive it. If this happen with the nitrate of lead, we always perceive it, because nitric acid is formed, which partly mixes with the water produced by the combustion, and partly passes over with the gas, which forms in consequence stains of oxide on the mercury. These circumstances occasioned me a great deal of trouble, for at the commencement of my experiments I employed the acetate of lead in preference, because it is so easily decomposed.

I analysed these vegetable salts with a base of lead in two ways :

1. By means of *sulphuric acid.* This method has the inconvenience of depending on the dexterity of the operator. The transvasations and filtrations of necessity occasion small errors in the result.

2. By *combustion.* I took a small capsule of glass, very thin. I weighed it, and put into it a determinate quantity of the salt to be analysed. I then heated the capsule by means of a spirit of wine lamp till all the vegetable matters were burnt. I then weighed it. The oxide of lead was then dissolved by distilled vinegar. I well washed the metallic lead left undissolved by the vinegar, and I weighed it anew. The weight of the lead shows how much oxygen must be added to the mass remaining after combustion to have the weight of the oxide of lead, and of consequence of the vegetable substances burnt. This method gives results so uniform that not unfrequently in three or four subsequent analyses I obtained exactly the same weights. This method, however, has an inconvenience, but it is not considerable. The combination is decomposed in such a manner as to produce in the first place a carburet of lead, which afterwards takes fire, and burns with such vivacity that a small quantity of the metal is driven off, and this causes a loss, which being added to the quantity of acid occasions a double error.

Article II.

A Geognostical Sketch of the Counties of Northumberland, Durham, and Part of Cumberland. By Thomas Thomson, M.D. F.R.S.

(With a Map.)

(Concluded from p. 346.)

II. The second formation to which I give the name of Newcastle Coal Formation, is of much more limited extent than the preceding. It begins at Morpeth, or a little to the north of it, and extends, I suppose, as far south as the river Tees; though of this I am not quite certain, as I did not examine that part of the country myself. It extends about ten miles west from Newcastle. Its western boundary in the county of Durham, as marked in the map, is probably near the truth, as it was pointed out to me by some of the mining agents, who had the best opportunity of being acquainted with that part of the country. This formation in fact possesses the characters of the Independent Coal Formation, as well as the preceding; but I was led to consider it separately, because the galena veins do not penetrate it; a demonstration that it must have been deposited at a very different time from the preceding formation.

The beds of which it is composed, like those of the preceding formation, dip towards the east, and crop out towards the west, so that in various places the beds of coal may be found at the surface. Though I was at considerable pains to procure information respecting this formation, and though many facts were stated to me by gentlemen on whose veracity and intelligence I think I may rely, yet I do not conceive myself so well informed respecting it as I am respecting the preceding formation. I shall state, however, what I conceive to be its structure.

The beds are 82 in number, and consist only of three different kinds of minerals; namely, *coal, sand-stone,* and *slate-clay,* which alternate with each other a great number of times. No lime-stone occurs in it, nor any green-stone or basalt, except in veins which traverse it, and which I shall afterwards notice.

1. It is not easy to determine the number of beds of coal, on account of the different depths at which the same bed occurs in different parts of the mining field, and on account of the various slips occasioned by the veins or dykes which traverse the formation. I conceive the whole number to amount to 25 beds; but a considerable number of these are insignificant in point of thickness. The two most important beds by far are those distinguished by the names of High Main and Low Main. The thickness of the first is six feet, that of the second six feet six inches. The Low Main is about 60 fathoms below the High Main. Between them occur eight

1   Independent Coal formation

2   Newcastle Coal formation

3   Magnesian Limestone formation
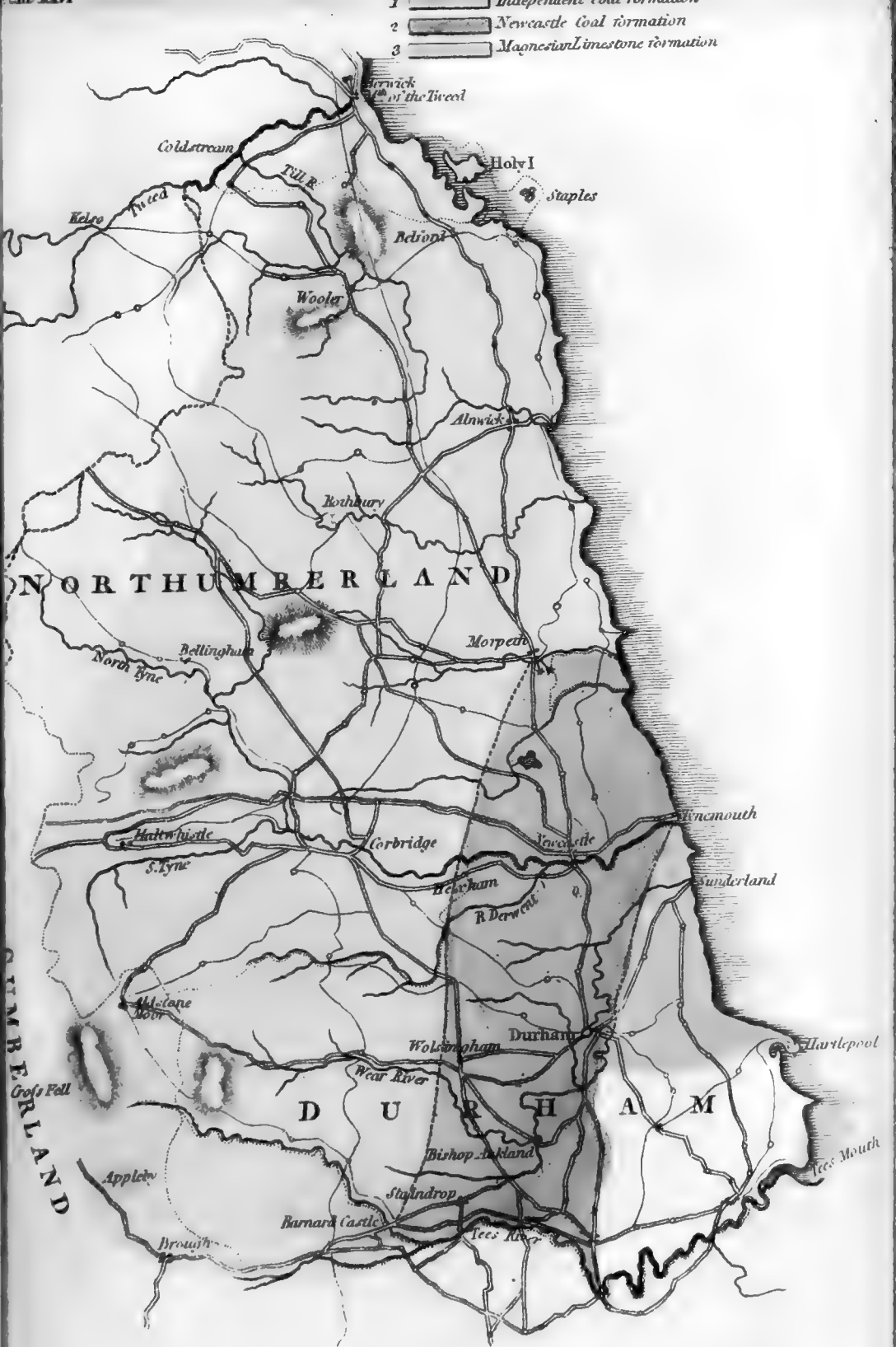
GEOGNOSTIC MAP of the COUNTIES of

Northumberland Durham and part of Cumberland.

Engraved by J.Sleury for Dr.Thomson's Annals Published by R.Baldwin Paternoster Row Dec.1.1814.

other beds of coal, one of which, called Coal Bensham, is four feet thick; and another, called Coal Yard Coal, is three feet thick. Seven beds of coal have been observed under the Low Main, some of which are of considerable thickness. The superior excellence of the coal found in this formation over every other is sufficiently known. The quantity of coal raised annually in this district, and sent to London, and the whole east and south coast of Great Britain, is quite enormous. Shields and Sunderland are the two places from which they are exported, and a curious distribution of the trade has taken place, depending upon the size of the two rivers. The Tyne vessels are larger, and therefore chiefly destined for the London market. The Wear vessels, on the contrary, are so small that they can make their way into the small rivers and harbours all over the kingdom, and therefore they supply the whole east and south coast as far west as Plymouth.

To form an idea of the quantity of coal contained in this formation, let us suppose it to extend in length from north to south 23 miles, and that its average breadth is eight miles. This makes a surface amounting to rather more than 180 square miles, or 557,568,000 square yards. The utmost thickness of all the beds of coal put together does not exceed 44 feet; but there are eleven beds not workable, the thickness of each amounting only to a few inches. If they be deducted, the amount of the rest will be 36 feet, or 12 yards. Perhaps five of the other beds likewise should be struck off, as they amount altogether only to six feet, and therefore at present are not considered as worth working. The remainder will be ten yards; so that the whole coal in this formation amounts to 5,575,680,000 cubic yards. How much of this is already removed by mining I do not know; but the Newcastle collieries have been wrought for so many years to an enormous extent that the quantity already mined must be considerable. I conceive the quantity of coals exported yearly from this formation exceeds two millions of chaldrons; for the county of Durham alone exports $1\frac{1}{3}$ million. A chaldron weighs 1·4 ton; so that 2·8 millions of tons of coal are annually raised in these counties out of this formation. Now a ton of coal is very nearly one cubic yard; so that the yearly loss from mining amounts to 2·8 millions, or (adding a third for waste) to 3·7 millions of yards. According to this statement, the Newcastle coals may be mined to the present extent for 1500 years before they be exhausted. But from this number we must deduct the amount of the years during which they have been already wrought. We need not be afraid, then, of any sudden injury to Great Britain from the exhaustion of the coal-mines. It is necessary to keep in mind likewise that I have taken the greatest thickness of the coal beds. Now as this thickness is far from uniform, a considerable deduction (I should conceive one-third of the whole) must be made in order to obtain the medium thickness; so that we may state in round numbers that this formation, at the present rate of waste, will supply coal for 1000 years; but its price will be con-

tinually on the increase, on account of the continually increasing expense of mining.

I have already on a former occasion stated my opinion respecting the mode of working the collieries practised here, and indeed all over the kingdom; and have seen nothing in my late visit to induce me to alter that opinion. Delicacy would have prevented me from venturing to state that opinion; but the numerous lives sacrificed to the present system, and the increase of the danger as the mines get deeper, render silence on the subject improper, if not criminal.*

2. The sand-stone in this formation is known by the name of

* From a letter in the Philosophical Magazine for October, p. 303, subscribed *An Engineer*, it would appear that some observations of mine in the *Annals of Philosophy*, vol. iii. p. 434, have been misunderstood by the class of men to whom they were chiefly directed. I shall therefore take this opportunity of stating them more explicitly. 1. Fire-damp has been repeatedly analysed by myself and others, and found always to consist of pure carbureted hydrogen gas without any mixture of sulphureted hydrogen gas. I have spent many hours in coal-mines without ever perceiving the smell of sulphureted hydrogen gas. These facts induce me to believe that sulphureted hydrogen gas is not formed in coal-mines. Now I do not see how iron pyrites (a compound of iron and sulphur) can contribute to the formation of carbureted hydrogen (a compound of carbon and hydrogen). But if this *Engineer* can show us in what manner pyrites can contribute to this formation, he will be in the right to say that the explanation of my Newcastle correspondent was very probable; but he has no right to draw any such inference till such an explanation is offered. 2. I spent 25 years of my life in a coal country, and in different parts of it, at Stirling, in Fife, in Mid-Lothian. During 12 years of that time I was interested in the subject of coal-mines, in consequence of the branch of science to which I had attached myself. I never during that time heard of a single explosion from fire-damp in any of these mines; yet if they had occurred I think I must have heard of it. About Glasgow explosions from fire-damp never take place. This is the case also at Tweedmouth; but explosions take place at Borrowstonness, at Newcastle, in Staffordshire, and in the neighbourhood of Bristol. Now in Mid-Lothian, at Stirling, Glasgow, Tweedmouth, the mines are shallow; at Borrowstonness, Newcastle, Staffordshire, Bristol, they are deep. What other inference can be drawn from this than that fire-damp only accumulates in deep mines. I conclude from it that fire-damp is formed very slowly. Weeks, probably months, or even years, elapse, before it is evolved in sufficient quantity to become dangerous. The fire-damp in deep coal-mines has probably been accumulating for ages; and unless the mode of ventilation were very imperfect indeed, it would make its escape before it became dangerous in point of quantity. 3. The *Engineer* observes that I do not seem to be aware that the accidental and progressive falling of the roofs of most coal-pits occasions numerous higher places, or dome-like cavities, above the old hollows, and even over the gates and passages in too many instances, wherein it is impossible to prevent the lighter gases, where they abound, from accumulating. Now I beg leave to inform this *Engineer* that I am perfectly aware of all this, and more than this; and that these are the very defects in the present system to which I alluded. As long as they exist it will be impossible to free coal-pits from fatal accidents from fire-damp. The coal engineers, I know, universally affirm that these are defects which it is impossible to remedy; but I own that I am not disposed to admit of such sweeping conclusions, nor to believe that the art of coal-mining has already reached its utmost limit of perfection. It would be convenient for these gentlemen to be provided with a demonstration of such impossibility of improvement. If they were to apply to Dr. Olinthus Gregory, who has a knack of demonstrating impossibilities, it is probable that he would supply them to their satisfaction. I admit without hesitation that the present system is so bad that it does not admit of remedy in old collieries; but this is no reason why new collieries should not be constructed on more scientific principles. I should think more highly of *An Engineer* than I do at present, if he would turn his attention to improving the present wretched mode of ventilating coal-mines, instead of giving his opinion on topics which he obviously has not examined.

post ; for it is curious that the name of all the rocks in the coal and lead mining districts are quite different from each other. The colliers and lead miners have little communication with each other. Their mode of working is quite different, so that they cannot supply each others' places. The colliers appear to my eye to be a stouter and healthier race of men than the lead miners. There are about 25 beds of sand-stone in the Newcastle Coal Formation, some of them of considerable thickness, but the greater number thin. The stone is usually fine-grained. It is soft, and not very durable when used as a building stone. Its colour is most commonly grey, with a shade of yellow; but its appearance must be familiar to every person who has visited Newcastle, as many houses in that town are built of it. There is a free-stone bed in the hill to the south of Newcastle, called Gateshead Fell, which makes excellent grind-stones. Accordingly almost all the grind-stones used in Great Britain, and indeed on the Continent also, are made here.

3. The slate-clay in this formation is called *metal-stone,* and is usually distinguished by prefixing the name of its colour. Thus some beds of it are called *blue metal-stone,* others *grey metal-stone.* When very much indurated it is called *whin* ; for I did not find this name applied to sand-stone by the Newcastle colliers ; probably because their sand-stone is always soft. The number of slate-clay beds is about 32 ; and they are usually thinner than the sand-stone beds with which they alternate. Both the sand-stone and slate-clay form the roof and floor of coal-beds, but the latter much more frequently than the former. I have observed both in immediate contact with the coal, without the smallest sensible alteration in the properties of this combustible substance.

4. The number of veins, or *dykes,* as they are called, traversing this formation is very considerable. They seem to run in all directions. The most celebrated of them all is called the Great Dyke, not in consequence of its size, which is very inconsiderable, but because the beds on the north side of it are thrown down 90 fathoms. Its direction is N. N. E. and S. S. W. It enters the sea a little to the south of Hartley, or about three miles north of Shields ; and, running westwards, crosses the Tyne at Lemington, nearly opposite to Blaydon, about four miles west from Newcastle Bridge. It continues in the same direction as far as it has been traced. This vein is only a few inches thick, and it is filled with soft clay and slate-clay.

There is another very considerable dyke, which appears first at the sea side at North Shields, and, running in nearly a due west direction upon the north side of the Tyne, passes through Chapel Hill, a little to the north of Newburn-on-the-Tyne. It has been traced still farther west in the same direction. This vein is filled with sand-stone, which I consider as a very curious circumstance. I am not sure that any similar vein has ever before been observed in a coal-field. It occasions no shift in the position of the beds, and on that account has acquired but little celebrity among the colliers ; but mineralogists, I flatter myself, will do it more justice.

There is another dyke, which occurs about ten miles west from Newcastle, and probably rises to some height above the surface of the ground; for it is known by the name of Coaley Hill. I did not myself see it; but I saw abundance of its contents as they are driven to Newcastle, and used as stuff for mending the roads. The substance was basalt, rather lighter coloured than usual. It contained large crystals of felspar, and very minute black crystals, which were probably augite. In some specimens there are round cavities filled with chalcedony. I was not so lucky as to observe any specimens of green-stone; though the existence of this basalt dyke renders it very probable that dykes of green-stone likewise occur in this coal-field. Various dykes occur between the rivers Tyne and Wear, and the beds in consequence are so much deranged that I have a strong suspicion that some confusion exists with respect to the identity of the different Tyne and Wear coal beds.

I was led to expect from various accounts which I had received that the coal in the neighbourhood of these dykes would be greatly altered in its quality, and would be similar in appearance to coke; but I was not lucky enough to witness any such alteration, though I examined the coal in the neighbourhood of various dykes, both at Tweedmouth and Newcastle, with considerable attention. Such occurrences, therefore, must be rare, and like the disappearance of the fluor spar in the galena veins, I am afraid that in the present state of our knowledge we cannot hope to be able to account for them in a satisfactory manner.

As the small coal cannot be sent to London, or sold to advantage on the spot, great quantities of it are often piled up near the mouths of the coal-pits. These masses of coal frequently take fire of themselves, and burn for a good many years with great brilliancy. Two such heaps in combustion may be seen at present on the north side of Newcastle. If you travel from Berwick to Newcastle, and enter this last town in the dark, about three miles from the town, you see two immense fires; one on the left hand, about three miles from the road, which has been burning these eight years. The heap of coal is said to cover 12 acres. The other, on the right hand, is nearer the road, and therefore appears more bright; it has been burning these three or four years. These fires are not visible during the day, but only during the night. It has often occurred to me that this small coal might be converted into coke with profit, and certainly in all cases where coal gas is wanted it would answer as well as any coal whatever.

III. The third and last formation of these counties is the Magnesian Lime-stone Formation. Though I now regard it as of considerable importance, I must acknowlege with regret that my examination of it was very superficial and imperfect. When I went to Sunderland I was not aware of the nature of the country, and I was so unwell that I found myself unable to travel over the country sufficiently to determine the requisite points. My account, therefore, will not only be very imperfect, but unsatisfactory, as far as the limits of this formation goes. I traced it from the river Tyne

along the coast some miles beyond Sunderland Bridge. I do not know whether it goes so far south as the Tees, but am inclined to suspect that it does. It may be even connected with the magnesian lime-stone of Yorkshire and Derbyshire, analysed many years ago by Mr. Tennant; but the determination of this point must be left to future observers. It seems to be broadest upon the banks of the Wear; but I do not conceive its greatest breadth to exceed four or five miles.

I do not know its thickness; I have seen quarries which appeared to my eye nearly 100 feet deep. The surface is uneven, rising into round backed hills, or rather knolls, for they scarcely exceed 20 or 30 feet in height. The thickness cannot any where be very considerable, as coals have been wrought under the magnesian lime-stone west from Sunderland. No pit indeed, as far as I know, has been sunk directly through the lime-stone to the coal beds; but coal beds have been followed and wrought after they have passed under this lime-stone.

To form some notion of the position of this rock, I examined four different hills, which had been long wrought to a great extent, and were therefore well exposed. Three of these hills were composed of distinct beds, dipping very gently to the south-east; but I could not perceive any appearance of stratification in the fourth hill. Whether this was owing to the action of the weather, for it alone of all the four was not wrought as a mine, or that no strata in fact existed, I cannot say; but I observed two other differences between them no less striking. The stratified hills had a crystalline texture, a glimmering lustre, and abounded in crystals, which on the spot I considered as swine-stone, and which, when rubbed, gave out a strong bituminous odour; but I cannot find that this is the case with any of the specimens which I brought with me. Hence I am unable to determine whether I was mistaken in my observations or not; and I wish that some gentleman on the spot would examine the matter anew. Building Hill is the place where these crystals most abound. I analysed some of them, and found them destitute of magnesia, or at least very nearly so. The hill, in which no stratification could be perceived, was composed of a porous soft stone, and was full of the casts of shells, corals, and other marine productions.

There is a very extensive quarry, called Fullwell, about a mile north of the Wear, at Sunderland Bridge. Above the rock there are a few feet of earth (probably decayed lime-stone), which is full of round lime-stone balls, of various sizes. They were sometimes quite spherical, and sometimes botryoidal. In the centre of each ball there occurs a small cavity, the walls of which are lined with minute crystals. The internal colour in some balls is light ochre-yellow, in others yellowish-grey; lustre, glimmering; fracture, even, splintery. There are evident traces of a tendency to crystallization, especially towards the surface of the ball, and then the lustre is more considerable; very solid, and difficultly frangible; hardness, that of limestone; specific gravity, 2·649. These balls,

though occurring in a magnesian lime-stone quarry, contained no magnesia, but consisted of carbonate of lime coloured with iron.

I find a considerable difference in the chemical composition of the magnesian lime-stone brought from different quarries. I shall therefore give a description and analysis of the different specimens I brought with me to London. My method of analysis was this :— The specimen was dissolved in nitric acid, and the loss of weight considered as carbonic acid. A quantity of sulphuric acid capable of saturating the same quantity of base as the carbonic acid driven off was now put into the liquid, and it was distilled to dryness in a retort. Distilled water was poured upon the dry residue, and allowed to remain till it had dissolved the sulphate of magnesia. The whole was then thrown upon a filter. The liquid that passed through was evaporated to dryness, exposed to a red heat, redis-solved in water, and a little alcohol being added to the liquid, it was again filtered. By these repeated filtrations all the sulphate of lime was separated. It was heated to redness, and weighed. The sulphate of magnesia, being evaporated to dryness, was also exposed to a red heat, and weighed. I conceive this method, though perhaps not rigidly precise, yet as coming very near the truth ; and it has the great advantage of being very simple, which in chemical analysis is a point of primary importance.

1. *Magnesian Lime-stone from Building Hill, near Sunderland.*

This hill constitutes a hard stratified rock, and is not the least porous. Colour white, with a considerable tint of ochre-yellow, chiefly passing through it in small stripes ; fracture, splintery ; lustre, dull, or scarcely glimmering ; fragments, indeterminate, sharp-edged ; translucent on the edges ; semi-hard ; readily scratched with a knife ; sp. gr. 2·791. This variety dissolves very slowly in acids, and therefore must be reduced to a fine powder in order to determine the proportion of carbonic acid which it contained. Its constituents were,

Carbonate of lime	56·80
Carbonate of magnesia..............	40·84
Carbonate of iron	0·36
Insoluble matter	2·00
	100·00

2. *Magnesian Lime-stone from Humbleton Hill, near Sunderland.*

This hill is composed of porous lime-stone, full of casts of shells, and marine remains, and has no marks of stratification.

Colour cream-yellow, here and there tinged ochre-yellow, and in one or two points I observed dots of peach-blossom red ; fracture uneven, in some places imperfectly conchoidal ; opaque ; hardness rather greater than that of calcareous spar ; scratched by fluor spar, but not readily ; fragments indeterminate, rather sharp-edged ;

lustre glimmering, from a few small crystals of calcareous spar, or bitter spar, interspersed; specific gravity 2·637; but as the stone is very porous, that was probably below the truth. Its composition was as follows :—

Carbonate of lime	51·50
Carbonate of magnesia............	44·84
Insoluble matter	1·60
Loss	2·06
	100·00

I find the carbonate of magnesia in these lime-stones composed of one atom carbonic acid and one atom lime. This variety dissolves in acids as rapidly as pure carbonate of lime.

I cannot find that any of the shells which occur in this rock have been hitherto described by conchologists. None of them are known to exist in a recent state upon our coasts. But that they are sea shells I consider as demonstrated by the impressions of flustras, which are visible in different parts of the stone, and by casts of corralines, which are not uncommon in it.

The most curious shell has some resemblance to a shell observed by Martin in the magnesian lime-stone of Bredon, in Derbyshire, and probably constituting a species of the same genus. Mr. Sowerby has, in his Mineral Conchology, Tables 68 and 69, given figures of various shells allied to it, most of which he got from Dr. Fleming, of Flisk, Fifeshire. Mr. Sowerby has constituted these shells into a new genus, to which he has given the name of *producta*. Our shell, then, seems to be a species of producta. Mr. Sowerby conceives that he can distinguish no fewer than casts of five species of producta in the specimen which I brought from Humbleton Hill.

Another very perfect cast appears to be a new species of pecten. There is a small cast of a shell which Mr. Sowerby thinks like the terebratula subrotunda. These, with various casts of encrini, are all the shells that I have been able to observe. From these shells it is obvious that this magnesian lime-stone was deposited at the bottom of the sea, and that, though the most recent of all the beds existing in the county of Durham, it must be of very ancient date, since the deposition must have taken place before the existence of the shell fish which at present live upon our coast.

3. *Flexible Lime-stone from Marsdon Rock, on the Sea Shore five Miles north of Sunderland.*

I did not visit this rock myself; but got the specimens of it, from which my examination was made, from Dr. Reid Clanny, of Sunderland, to whose mineralogical zeal I am indebted, not only for my knowledge of the existence of this curious rock, but for the opportunity of examining all the spots which attracted my attention in the neighbourhood of Sunderland. All the specimens which I

got had been cut into long thin plates, and consisted of two layers, each about one-eighth of an inch thick, lying upon each other, but admitting of an easy separation. These thin plates are very flexible. When held by the two extremities the middle bends several inches; but it does not possess the smallest elasticity.

Colour cream-yellow, differing in intensity in different parts; some specimens have nearly a tile-red colour, and here and there streaks or bands of this colour occur in the cream-yellow similar to what we observe in ribbon jasper.

Fracture even, earthy; soft; cannot be scratched by the nail, but yields very easily to the knife; the fracture in the great is probably slaty.

Lustre dull; opake; fragments indeterminate; specific gravity 2·541; but as the stone is porous, this is probably below the truth. Its composition I found as follows :—

Carbonate of lime	62·00
Carbonate of magnesia	35·96
Insoluble matter	1·60
Loss	0·44
	100·00

It dissolves in acids as readily as common carbonate of lime.

This magnesian lime-stone has been long burnt in prodigious quantities in the neighbourhood of Sunderland, and sent coastways both to the north and the south. It goes in great abundance to Aberdeenshire. As no complaints have ever been made of its being injurious when employed as a manure, it would be curious to know whether this circumstance be owing to the soil on which it is put, or to the small quantity of it used, in consequence of its price, occasioned by the long carriage; for it appears from Mr. Tennant's statement that at Ferrybridge the farmers are aware that it does not answer as a manure so well as pure carbonate of lime.

In the preceding rapid sketch I have taken no notice of some small patches of the newest floetz trap which occur towards the north-east parts of Northumberland. I examined several of these places about four years ago, and found them to consist of greenstone rocks seemingly deposited above the Independent Coal Formation. This is the case with the rock on which the castle stands in Holy Island. The basis of this island is lime-stone. The same thing occurs at Bamborough Castle, and in several hills in the neighbourhood of Belford. These facts may have some interest to the geologist, though I did not consider them as of sufficient importance to interrupt the very general view of the structure of these counties which I have now given.

I shall terminate this essay with a list of some of the most remarkable terms connected with mineralogy peculiar to the counties of Northumberland, Cumberland, and Durham; and I shall give

their meaning in the common mineralogical language which has been adopted in this country. Such a list may be of some little service to future mineralogists :—

Flats .... A swelling out of the vein.
Forehead. .... Extremity of the working of a mine.
Girdle ..... A thin hard bed of slate-clay or sand-stone.
Groove* .... A mine.
Hazle ...... Sand-stone.
Metal-stone.. Slate-clay.
Plate ...... Slate-clay.
Post .... Sand-stone.
Rider ...... The upper part of a vein.
Scarr ...... A rock.
Shiver...... Slate-clay approaching to shale.
Sill .... A bed.
Sulphur .... Iron pyrites.
Thill ...... Clay.
Whynn ..... Hard sand-stone or slate-clay.

These I conceive to be the most striking terms, and the most apt to puzzle strangers.

Article III.

Mineralogical Observations. By Professor Jameson.

A KNOWLEDGE of the various appearances presented by cotemporaneous imbedded masses and veins, and of the relations of beds and strata to each other, not only facilitate mineralogical investigations, but also aid us in our geognostical speculations. In the second volume of the Memoirs of the Wernerian Society I have already touched on these subjects; and I shall now add some facts to those already stated in that work.

I.—*On Cotemporaneous Masses.*

1. In granite mountains we sometimes observe portions of gneiss, mica-slate, clay-slate, and also of porphyry, syenite, and trap, varying in size from a few inches to many fathoms. These masses are to be seen passing by imperceptible shades into the bounding granite; thus showing that they are of cotemporaneous formation with it, and are not fragments, as has been frequently maintained.

* This is exactly the Swedish word *grufve*, which has the same meaning, and almost the same sound.

Appearances of this kind occur in the granite of Braemar, of Aberdeen, and other districts in Scotland.*

2. In gneiss districts we occasionally observe masses of granite in the gneiss, from a few inches to many fathoms in extent. These vary in shape, being round, oval, sharp-angular, blunt-angular, or with long arms shooting from them into the bounding rock in the form of veins. They are often observed passing into the bounding gneiss ; a proof of their being of cotemporaneous formation with it. Interesting appearances of this kind occur in the bold rocky coast which extends from Aberdeen to Finen. In the country immediately above this coast we meet with hillocks of granite rising through the gneiss ; these are cotemporaneous masses of granite in the gneiss, and not portions of the older or central granite.†

3. In districts composed of syenite we frequently observe imbedded portions of porphyry and trap. These vary in size, from a few inches to many fathoms, and pass gradually into the bounding syenite ; thus proving that they are of cotemporaneous formation with it. The rocky coasts around Peterhead, the syenite of Glen Tilt, and the syenite hills of Galloway, afford fine examples of these appearances. It is of consequence to attend to the distinction here pointed out, otherwise we run the risk of confounding together in our descriptions the syenite, porphyry, and trap formations, and might be led to describe a country as composed of three formations, when in reality it contained but one. Such mistakes can scarcely be committed when we have a coast to describe where the appearances are almost always well exposed ; but in the interior, where the rocks are much hid, the error has been fallen into.

4. In mountains of coarse granular granite we frequently meet with variously shaped imbedded portions of small granular granite. These at first sight might be confounded with fragments ; but the fact of many of them passing into the bounding rock shows that they are cotemporaneous formations. It is worthy of remark, that these imbedded portions have sometimes a globular or oval form, but a slaty structure ; and that, when many of them occur together, an appearance is formed precisely like that exhibited by topaz rock ; that is to say, the mass is granular in the large, and slaty in the small.‡

5. Masses of lime-stone of considerable purity occur in some districts imbedded in calcareous sand-stone ; these are sometimes of considerable magnitude ; and being in general more compact than

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* It is probable that some hills of gneiss, porphyry, &c. in granite districts, are cotemporaneous masses imbedded in the granite, and portions of the gneiss or porphyry formations.

† We sometimes meet with veins of compact gneiss or of granite so dark coloured that they might be confounded with basalt, or some rock of the trap series. I suspect some of the trap veins described as occurring in granite are of this nature.

‡ The topaz rock may be mentioned as an example of a rock formed by crystallization, yet having a striking brecciated or conglomerated aspect.

the sand-stone, are observed projecting from the strata where the rocks have been much exposed to the action of the weather. Masses of this kind at first sight appear to be fragments of lime-stone; but I satisfied myself that such of them as I had an opportunity of examining are of cotemporaneous formation with the sand-stone, because they exhibit appearances similar to those presented by granite in gneiss and gneiss in granite. This calcareous sand-stone is met with in East Lothian.

6. The same bed of sand-stone occasionally varies much in colour and hardness; thus one part of it will be red, and rather soft, serving as a basis in which white and harder portions are contained; or the general mass of the bed will be white and soft, and include harder masses of a red variety. These included portions, like the masses of granite in gneiss, vary in shape, being round, oval, angular, with projecting arms like veins; and they have either the same simple granular structure with the sand-stone in which they are contained, or they have a granular structure, and the sand-stone in which they are contained a slaty structure; thus affording an example in sand-stone of an analogous appearance to that observed in gneiss when it includes cotemporaneous portions of granite. We may mention the sand-stone of Arran and of East Lothian as affording instances of the appearances just described.

7. In strata of sand-stone, as in those belonging to the first floetz or old red sand-stone, and the coal formation, we sometimes meet with imbedded portions of porphyry, amygdaloid, basalt, green-stone, and trap tuff. These vary in shape and magnitude, and are to be observed passing imperceptibly into the surrounding sand-stone; thus showing that they are cotemporaneous, not fragmented masses. When the imbedded trap rocks are very compact, and the sand-stone comparatively loose in its texture, it sometimes happens, particularly on sea coasts, that the softer sand-stone is partly carried away, and the harder, or trap rocks, appear rising through the sand-stone, just as we observe harder sand-stone rising through softer, a granite through gneiss, and might be confounded with the outgoing or crop of a vein, or some other repository. There are examples of this appearance in different places on the sea coast of Scotland.

8. The slate-clay of the coal formation, as well as that which occurs in the old red sand-stone, occasionally alternates with beds of green-stone, and sometimes it contains imbedded portions of that rock. These portions have all the marks of cotemporaneous formation, and therefore bear the same relation to the slate-clay in which they are contained as the granite masses already mentioned bear to the gneiss. There is an example of this appearance near Aberdour, in Fyfeshire.

9. In the old red sand-stone of the middle division of Scotland,[*]

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* The middle district includes that part of Scotland which is contained between the Firth of Forth and the chain of lakes extending from Inverness to Fort William.

in that of Dumfrieshire, and the Island of Arran, there are beds of a compact splintery lime-stone, which occasionally contain imbedded masses of lime-stone, varying in magnitude and figure; and even whole beds occur principally composed of these masses, and have thus a brecciated or conglomerated aspect. That the imbedded portions are not fragments, and consequently the conglomerated beds not of a mechanical nature, may be inferred from this fact, that an uninterrupted transition is frequently to be observed from the uniform compact lime-stone to the angular concretions or fragments, resembling what is observed in the transition from compact into granular lime-stone, or from compact into granular galena.

10. In the great beds of trap tuff, that occur in the old red sand-stone and the coal formations in the middle and southern divisions of Scotland, we sometimes observe globular and other shaped masses of tuff, which in a superficial view have much the appearance of fragments or rolled masses. A careful examination of them shows that they are of the same general nature with the basis in which they are contained; further, that they pass gradually into that basis; so that it is sometimes difficult to point out the line of separation between the basis and the imbedded mass. Hence we infer that they are not fragments, but formed at the same time with the inclosing rock.

11. Lime-stone sometimes occurs in the form of beds in slate-clay, and occasionally imbedded in it in masses, varying in size and shape. The imbedded masses are sometimes so much intermixed with the slate-clay that an uninterrupted transition is to be traced from the one into the other; in other instances the imbedded masses are distinctly separated from the slate-clay at their line of junction, and have very much the appearance of fragments; but these are not fragments, as is evident from their forming one extremity of a series, the opposite extremity of which is calcareous, or even pure slate-clay.

12. Sand-stone occasionally occurs in imbedded masses in slate-clay, exhibiting the same geognostic relations with the lime-stone and slate-clay just mentioned. Hence these masses are to be considered, not as fragments, but as having been formed at the same time with the inclosing slate-clay.

II.—*On Cotemporaneous Masses and Veins occurring together in the same Mass of Rock.*

We sometimes observe within a short space, as a few hundred yards, in the same tract of country, beds, imbedded masses, and cotemporaneous veins, of one rock in another: thus in primitive mountains we occasionally meet with cliffs of which the predominating rock is gneiss, subordinate to which there occur beds, imbedded masses, and cotemporaneous veins of granite; in other instances the predominating rock is granite, in which there occur beds and imbedded portions of gneiss.* Appearances of this kind

* The cotemporaneous masses of gneiss in granite appear occasionally to

are to be seen in the gneiss and granite of Aberdeenshire. Similar phenomena occur in floetz districts : thus we occasionally meet with beds, imbedded masses, and cotemporaneous veins, of amydgdaloid or trap tuff in a mass of sand-stone of no very great extent, and conversely similar repositories of sand-stone occur in masses of tuff or amygdaloid. In such cases the solution appears to have contained at the same time all the ingredients of which the different rocks are composed, and these have been separated from their menstruum into the form they now possess by specific attractions taking place amongst the particles. Were we inclined to venture farther in the hazardous field of geological speculation, we might conjecture that cotemporaneous formations taking place still more on the great scale might have given rise to the conical and tabular-shaped hills of certain floetz formations, and that the figures of many hills composed of rocks formed from a state of solution depend in some degree on their mode of crystallization.

III.—*On the Intermixture of Beds and Strata at their Junctions.*

The opinion held by some observers that strata and beds are seldom intermixed at their line of junction, and that transitions from one bed into another very rarely occur, cannot be considered correct, as will appear from the following statement :—

1. Beds of granite in gneiss are sometimes distinctly separated from the bounding rock ; in other instances an uninterrupted transition is to be observed from the granite into the gneiss, or the granite is interrupted with the gneiss at their line of junction. Similar appearances are to be observed in the beds of gneiss that occur in granite.

2. Beds of granular quartz, much resembling sand-stone, sometimes alternate with granite : these beds are either distinctly separated from the granite at their line of junction, or they are intermixed, and veins of the granite shoot into the quartz, and veins of the quartz into the granite. Alternations of granite and quartz occur in the district of Braemar, in Aberdeenshire.

3. Beds of green-stone frequently occur in clay-slate : sometimes they are distinctly separated from the clay-slate at their line of junction with it ; in other instances the two rocks are intermixed at their line of junction ; and occasionally a gradual transition is to be observed from the green-stone into the clay-slate.

4. Beds of hornblende-rock and hornblende-slate in gneiss and mica-slate are either distinctly separated from the bounding rock, or they are intermixed with it at their line of junction ; and sometimes it is intermixed with hornblende, and gradually passes into the hornblende rocks.

have the stratified structure, and the strata are variously inclined, a fact which shows that the slaty and stratified structures are sometimes independent of successive deposition, and that variously inclined strata can be formed independent of any inequality of the fundamental rock, or without the aid of a subterraneous power.

5. Beds of granular lime-stone in granite, gneiss, mica-slate, and clay-slate, are sometimes intermixed with these rocks at their line of junction, or pass by almost imperceptible gradations into them, particularly into the gneiss, mica-slate, and clay-slate.

6. Beds of granite in grey-wacke are either distinctly separated from it at their meeting, or they are much intermixed; and it frequently happens that there is a beautiful and distinct transition from the granite into the grey-wacke.

7. Beds of flinty-slate frequently occur in clay-slate : sometimes the two rocks are distinctly separated ; sometimes they are intermixed at their junction ; and in other instances we observe the clay-slate becoming gradually more silicious, and at length passing into the flinty-slate.

8. Beds of granular quartz or sand-stone occur in clay-slate : these beds in general are separated from the slate at their junction ; but instances occur where they are intermixed, and also where the slate becomes gradually more silicious, and at length passes into the sand-stone.

9. Beds of amygdaloid, sometimes of vast magnitude, occur in the old red sand-stone formation ; the amygdaloid is occasionally intermixed with the sand-stone where they meet, or there is a gradual transition from the one rock into the other.

10. Beds of trap tuff occur in the old red sand-stone and coal formations ; these, like the beds of amygdaloid already mentioned, are either distinctly separated from the bounding rock, or they are intermixed with it, or gradually pass into it.

11. Green-stone occurs frequently in beds in the old red sand-stone and coal formations, and exhibits precisely the same relations as the amygdaloid, trap tuff, &c.

Article IV.

*On some Unknown Combinations of Chromic Acid with different Bases.** By Professor John.*

I undertook these experiments with a view of filling up some gaps which still exist in the table of chromates; but I must acknowledge that they do not possess the accuracy which might have been obtained by performing the experiments upon a larger scale. When we attempt to crystallize salts in small quantities we have often to struggle with insurmountable difficulties; and even in the most favourable circumstances, it is very difficult to determine the state of small, and often imperfectly formed, crystals.

1. *Chromate of Soda.*

Chromic acid neutralized with soda forms a dark yellow solution, which by spontaneous evaporation forms thin six-sided tables with two long and four short faces. They are transparent, easily soluble in water, and do not alter vegetable blue colours. They are not entirely insoluble in alcohol, but require a considerable proportion of that liquid to dissolve them.

2. *Chromate of Potash.*

Chromic acid dissolves potash, and at the same time lets fall a small quantity of green oxide.* The orange-coloured solution has a great tendency to efflorescence, especially when little pieces of wood are introduced into it.

Chromate of potash, when rapidly evaporated, forms a yellow saline mass; when more slowly evaporated, it forms four-sided tables; they are not altered by exposure to the air, are transparent, and have an Aurora-red colour.

3. *Chromate of Ammonia.*

This compound effloresces, and forms dendritical crystals, while a brown powder separates, which is oxide of chromium. It is formed in consequence of the decomposition of the chromic acid by the ammonia, and appears as often as the salt is dissolved in water, and allowed to effloresce. When this salt is exposed to a red heat, it is completely decomposed.

4. *Chromate of Glucina.*

Glucina thrown down by carbonate of potash was dissolved very slowly by concentrated chromic acid; the solution has a yellow colour, and does not seem capable of crystallizing.

5. *Chromo-Sulphate of Glucina.*

When sulphate of glucina is poured into chromic acid, the appearance of the solution is not altered; but when the liquid is evaporated, there remains behind a triple salt in dendritical crystals in a state of efflorescence.

6. *Chromate of Yttria.*

Chromic acid dissolves yttria, cold, in considerable quantity, and with effervescence. The solution has an astringent and pungent taste, and, like most of the chromates, has an orange-red colour passing into yellow. The solution is quite neutral. Chromate of yttria gives by evaporation dendrites, which are composed of very

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* This oxide was without doubt dissolved in the chromic acid, and owed its formation to some accident, probably the filtration of the acid through paper. When this green oxide is separated by the filter, no new precipitate appears. Ammonia, on the contrary, precipitates the chromic acid.

fine and firm crystals, which, if I may be allowed a comparison, are similar to a tree loaded with fruit. These crystals, which consist of prisms and cubes, have a peculiar appearance. Chromate of yttria is very soluble in water.

7. *Chromate of Strontian.*

This salt is insoluble. When carbonate of strontian is thrown into chromic acid, a yellowish powder is formed, which is this salt : probably it would be most easily procured by double decomposition.

8. *Chromate of Barytes.*

Carbonate of barytes is not dissolved by chromic acid; but the earth combines with the acid, and forms a light yellow insoluble powder, which lies at the bottom of the vessel. This salt is best obtained by mixing together muriate of barytes and chromic acid.

9. *Chromate of Nickel.*

Chromic acid dissolves the carbonate of nickel in considerable quantity ; but after some hours a pulverulent precipitate falls from the clear solution, which is again soluble in an excess of acid. This precipitate is probably a chromate of nickel. The acid solution gives, by slow evaporation, fern-leaved crystals in the state of eliptical plates, truncated on both sides, which towards the beginning and end of the leaf stalks become smaller, in order to preserve the resemblance. When these crystals are exposed to a high temperature, the acid is decomposed, and a black mass formed, which is insoluble in water, and consists of oxide of chromium and oxide of nickel.

10. *Chromate of Cerium.*

Carbonate of cerium is dissolved in great abundance by chromic acid. The solution has a yellow colour, and an astringent taste ; after some time the chromate of cerium falls to the bottom as a yellow powder. The remaining solution deposites some small reddish transparent crystals, which make their appearance in the midst of an incrystallizable mass.

11. *Chromate of Tellurium.*

Tellurium likewise is dissolved by the acid : of all the chromates, this salt appears least disposed to crystallize. It deposites some soft round grains, but the greatest part goes into the state of a syrupy mass.

12. *Chromate of Uranium.*

Carbonate of uranium is readily dissolved with effervescence by chromic acid. The solution is coloured yellow, has an astringent taste, and the salt is easily dissolved in water. It shoots into a dendritical mass containing small cubic crystals. They have an aurora colour. This salt melts in a weak red heat, and after cooling appears of a dark brown colour. This compound does not

appear to be so easily decomposed as chromate of nickel; for the brown mass dissolves in water, except a small residue, which is a mixture of oxide of chromium and oxide of uranium. The solution has a yellow colour, and potash precipitates from it yellow oxide of uranium.

ARTICLE V.

On the Aurora Borealis. By Thomas Thomson, M.D. F.R.S.

THERE are few phenomena which have more attracted the attention of the curious than the Aurora Borealis, and scarcely any concerning the nature and origin of which we are more completely ignorant. I conceive, therefore, that it will be attended with utility to collect into one point of view the principal facts that have been ascertained respecting it, and lay them in their naked simplicity before my philosophical readers :—

1. This phenomenon was considered by the ancients as a preternatural appearance, which prognosticated some great event, or some important change in the country over which it was visible. It was usually described as armies of horse and foot engaged in battle in the sky. In this way we find it mentioned as appearing before the death of Julius Cæsar, and it was universally considered as foretelling that important event. For more than a year before the siege and destruction of Jerusalem by Titus Vespasian, the Aurora Borealis was very frequently visible in Palestine, and it is minutely described by the historian Josephus as one of the most remarkable prognostics of the disasters that were to follow.

Even as late as the reign of Queen Elizabeth, during which this phenomenon was not uncommon in England, it is described under the denomination of *burning spears,* or some similar appellation obviously alluding to the old opinion that it represented hostile armies engaged in battle. They were seen at London on January the 30th, 1560, and on the 7th of October, 1564. Camden and Stowe inform us that they were seen on the 14th and 15th of Nov. 1574. These, as far as I know, are the only instances upon record of their being observed in England during that period ; but several foreign writers mention their appearance pretty much about the same time. Thus Cornelius Gemma, Professor of Medicine in Louvain, mentions them under the name of *chasma,* as appearing in Brabant on the 13th February and 28th September, 1575. Michael Mæstlin, celebrated as the tutor of Kepler, informs us that during the year 1580 he saw these *chasmata,* as he calls them, twelve different times in the course of one year, at Baknang, in the country of Wirtemberg.

Gassendi describes a phenomenon of the same kind which was

seen all over France on the 2d of September, O. S. 1621, and he describes it by the name of *Aurora Borealis*; being, as far as I know, the first who applied that name, now so well known, and generally received.

2. From that period till the year 707 there is no mention of any Aurora Borealis having been seen either in Britain or on the Continent, though the number of observers during a considerable part of that period was very considerable, and the motives for publication, in consequence of the establishment of the Royal Society, and various similar institutions in different parts of Europe, were greatly increased. Hence there is every reason to believe that the Aurora Borealis was very uncommon during almost the whole of the seventeenth century. On November the 10th, 1707, an Aurora was seen in Ireland by Mr. Neve, and is described as such in the Philosophical Transactions. During the same year Olaus Romer observed the same appearance three several times at Copenhagen. The bishop of Hereford observed another in the night between the 9th and 10th of August, 1708, and communicated an account of it to the Royal Society. At last, on the 6th of March, 1716, a very splendid one was seen by Dr. Halley at London. He observed it with great attention, and has published a very minute description of it, together with an hypothetic explanation, in the Philosophical Transactions for 1716, vol. xxix. p. 406.

Since that period they were seen very frequently both in Britain and in every other part of Europe; and innumerable descriptions of them have been published. Indeed no phenomenon has been more familiar to meteorologists; and hence probably the reason why, like some other very common appearances, (the fall of hail, for example,) so very little progress has been made in explaining it. It was very common about the year 1792, and continued so for several years afterwards; but for some years past it has become rare again. Hence it is very likely that during the nineteenth century it may be as uncommon a phenomenon as it was during the seventeenth.

3. The appearance of the Aurora Borealis is so familiar to most persons at present that a minute description of it scarcely seems necessary. It consists of beams of a very pale light always situated in the northern part of the sky, and seemingly verging towards a point situated not very far from the vertex. These beams are constantly shifting their places by sudden flashes, so that the whole appear as if they were in motion. Hence the name *streamers* and *merry dancers*, by which they are distinguished in different parts of Scotland. Other appearances are frequently to be seen besides the beams, but these are constant, and I conceive them to constitute the essential part of the phenomenon.

4. The Aurora Borealis is observed in every kind of weather, and in every season of the year; though winter being the season most favourable for seeing them, we may suppose that a greater number have been observed during that part of the year than during summer, when the shortness of the night renders it particularly difficult to

observe them. In Scotland they are considered as rather indicating fair weather, though this is a prognostic which I cannot pretend to authenticate from my own observations, for I have seen every kind of weather follow them.

5. From the observations of Mr. Cavendish and Mr. Dalton, I think there can be no doubt that the arched appearance of the Aurora Borealis is merely an optical deception, and that in reality it consists of a great number of straight cylinders parallel to each other, and to the dipping needle at the place where they are seen. Mr. Dalton, indeed, has given a mathematical demonstration of this in his Meteorological Essays, p. 160, to which I beg leave to refer such readers as have not considered the subject with the requisite attention.

6. The height of these beams above the surface of the earth is much greater than that of most other meteorological appearances. There are two ways of calculating that height : one by means of a single observation, first explained by Mayer in the fourth volume of the Petersburgh Acts. It requires only the knowledge of the latitude of the place of observation, the apparent altitude of the Aurora, and the distance of the limbs of the arch in the horizon. Let P D p I of the annexed diagram be the meridian of the place, P p the axis of the earth, D I the diameter of the equator, N the summit of the Aurora, which is supposed to be situated in the plane of the meridian. Let O be the place of the observer, and H R the common section of the meridian and horizon. Now let us suppose P M = a, sine of N O R = m, the co-sine of half the distance of the limbs = g, the sine of the whole distance = r, the sine of P O = co-sine of the latitude = q, the sine of (90° + the latitude − the arch of apparent altitude) = p, and O N = y. Mayer has demonstrated that

$$y = \frac{2\,m\,a\,g^2\,q^2}{p^2 - g^2\,q^2}$$

This formula is too tedious for common use ; but Kraft, by a very ingenious transmutation, has reduced it to logarithms ; for if the numerator and denominator of the fraction be divided by p^2 we obtain

$$y = 2\,m\,a\,\frac{\dfrac{g^2\,q^2}{p^2}}{1 - \dfrac{g^2\,q^2}{p^2}}$$

Now the fractional part of this equation is nothing else than the

square of the tangent of the angle whose sine is $\frac{g\ q}{p}$. Therefore if we call this angle t, the formula is changed into the following :—
log. y = log. 2 + log. a + log. m. + 2 log. t − 3 log. radius.

Notwithstanding the ingenuity of this method, there are but few observations that have been made which are adapted to it. The other method consists in observing the position of the Aurora from two different places; and as this is universally understood it would be superfluous to give a detailed description of it here.

The following table exhibits the height of the different Auroræ Boreales above the surface of the earth that have been hitherto measured, as far at least as I have met with them in the writings of different philosophers :—

Date.	Place.	Observer.	Height in English Miles.
1621, 12 Sept.	Peinier	Gassendi	566
1726, 19 Oct.	Rome / Copenhagen	Bianchini / Horrebow	514
1730, 15 Feb.	Geneva / Montpellier	Cramer /	440
——, 16 March	Petersburgh	Kraft	$133\frac{1}{3}$
——, 6 Sept.	Petersburgh	Kraft	160
——, 2 Nov.	Geneva		456
1731, 8 Oct.	Copenhagen / Breuille	Horrebow / De Mairan	686
1732, 1 Sept.	Paris / Copenhagen	Buache / Horrebow	590
——, 12 Nov.	Paris / Copenhagen	Godin / Horrebow	480
1734, 22 Feb.	Paris / Copenhagen	Godin / Horrebow	580
1735, 22 Feb.	Paris / Copenhagen	De Mairan / Horrebow	454
1736, 22 Dec.	Paris / Torneo	De Fouchy / Celsius	534
1737, 16 Dec.	Paris / Montpellier	De Fouchy / Plantade	553
1740, 3 Nov.	Upsala / Havre de Grace	Celsius / De Mairan	433
1750, 3 Feb.	Paris / Toulouse	De Fouchy / D'Arquier	476
——, 3 Feb.	Paris	De Fouchy	466
——, 27 Feb.	Paris / Hague	De Mairan / Gabry	463
1751, 23 Oct.	Upsala	Fernerus	1006
1759, 4 Feb.	Upsala / Hernosand	Bergman / Gisler	534

Date.	Place.	Observer.	Height in English Miles.
1759, 15 Feb.	Upsala .... Hernosand	Bergman ...... Gisler	866
1760, 1 Nov.	Upsala .... Hernosand	Bergman ...... Gisler	666
——, 6 Nov.	Upsala .... Hernosand	Bergman ...... Gisler	534
——, 8 Nov.	Upsala .... Hernosand	Bergman ...... Gisler	334
1761, 25 Jan.	Hernosand ...... Lidköping	Gisler .... Bergman	334
——, 21 Feb.	Vienna .... Hernosand	Hell .... Gisler	720
——, 27 Feb.	Hernosand ...... Enköping	Gisler .... Bergman	334
——, 25 Sept.	Hernosand ...... Mariestadt	Gisler .... Bergman	666
1763, 24 Oct.	Upsala .... Christiana	Bergman ...... Piscator	334
1764, 22 Feb.	Upsala	Bergman	254
1784, 23 Feb.	London	Cavendish	62
1793, 15 Feb.	Kendal .... Keswick	Dalton .... Crosthwaite	150*

From this table it is obvious that the height of the cylindrical luminous bodies, which constitute the Aurora Borealis, varies very considerably at different times; but that they are always situated beyond the sensible atmosphere which surrounds the earth.

7. During the prevalence of the Aurora Borealis the magnetic needle is frequently observed to become unsteady.

These are the facts respecting the Aurora Borealis that seem to be perfectly authenticated. There are some other curious observations on it, which were made by Ritter; but I forbear to state them, because I do not know how far their accuracy can be depended on.

It is quite obvious from the preceding detail that the Aurora Borealis is connected with the magnetism of the earth; that the luminous cylinders are in fact magnets parallel to the magnetic axis of the earth, and pointing to the northern magnetic pole of the earth. Hence the position of the beams constituting the Aurora varies with the declination of the needle, as becomes quite obvious when we compare together the different observations of Auroræ made at different times.

The connection between the Aurora Borealis and magnetism was seen by Dr. Halley, who in his first paper on the subject, printed in the Philosophical Transactions for 1716, endeavours to explain

* This table, except the last two heights, is taken from Bergman, Opusc. v. 291.

the appearances of the Aurora, by supposing them produced by the *magnetic fluid* which he conceived to be constantly issuing out of the earth, and entering into it in lines, which he thought coincided with the position of the beams of the Aurora Borealis.

What the nature of the beams or of the cylinders is which constitute the Aurora Borealis, we have no means of knowing; neither can we explain the origin of the light which renders them luminous, though it would appear to be connected with electricity; but that these cylinders are magnets cannot be for a moment doubted. At present we know only three bodies capable of assuming magnetic properties; namely, iron, nickel, and cobalt. That the beams of the Aurora Borealis should consist of particles of any of these metals is too monstrous a supposition to be for a moment believed. At the same time, when we call to mind the luminous meteors which are occasionally seen at vast heights above the surface of the earth, and the stony bodies which there is every reason to believe occasionally fall from these meteors, the conjecture, that bodies similar in their nature to some of the solid bodies which constitute our globe may exist in some unknown state in the atmosphere, will not appear altogether extravagant.

ARTICLE VI.

Remarks an Mr. Hume's Paper on Barytes, contained in the Philosophical Magazine, vol. xiv. 1802. By Mr. R. Phillips.

HAVING had occasion to consult various authorities respecting the chemical properties of barytes, I was much surprised on reading the contents of a paper on the subject by Mr. Hume, printed in the Philosophical Magazine for 1802, but which I do not remember to have seen until very lately.

In this memoir some facts are stated, which the author says he " had never yet seen pointed out by any chemical writers; " and others are mentioned, which he asserts had never been previously " enumerated by any author."

The statements which I shall first notice are, " that nitrate of barytes, not in crystals only, but even a saturated aqueous solution, is perfectly insoluble in nitrous acid of the usual specific gravity; " and " that muriate of barytes is virtually insoluble in muriatic acid."

Now that these facts were well known, and had been described previously to the publication of Mr. Hume's paper, will appear by the following quotation from a memoir by Sage, contained in vol. lxxxviii. p. 145, of the Mémoires de l'Académie des Sciences for 1788 :—" L'acide nitreux à trente deux degrés, fait d'abord une vive effervescence avec le spath pesant aéré : le nitre qui en

resulte demandant beaucoup d'eau pour sa dissolution, se précipite aussitôt qu'il se forme, comme l'a observé M. Klaproth."

" L'acide marin dissout avec effervescence le spath pesant aéré : le sel qui en résulte se précipite aussitôt. L'acide marin qui surnage, ne tient point en dissolution de sel à base de terre pesante."

The next discovery claimed by Mr. Hume to which I shall advert is, that " sulphate of strontian has a capacity for super-saturation, forming an acidulous sulphate in solution, and decomposable by water;" and of this fact he asserts that " no author has given any account."

That this statement as to sulphate of strontian is correct, and the assertion respecting it erroneous, is proved by the following extract from p. 334, vol. i. of the English Translation of Klaproth's Analytical Essays, published in 1801 ; and the original was printed in Crell's Annals for 1795 :—" Upon 60 grains of pulverized stron-tianite, introduced into a retort, I poured by degrees two ounces of concentrated sulphuric acid. The first portion that was effused caused a great frothing. The contents of the retort were then brought to boiling in a sand heat. After cooling, the earth was found entirely dissolved, and the solution colourless ; but it is again decomposed as soon as any water is added."

The remaining parts of Mr. Hume's paper which I shall notice are contained in the following quotations :—" Sulphate of barytes is completely soluble in sulphuric acid ; forming a saline fluid or acidulous sulphate, analogous, in some of its characters, to phosphate of lime and many other salts, with capacity for excess of acid ; decomposable by water alone, which returns it to simple sulphate ; and this salt," says Mr. Hume, " never has been enumerated by any author."

" Carbonate of barytes is also totally decomposed by, and soluble in, sulphuric acid, forming, of course, the same acidulous sulphate. Respecting any figure this new salt may put on, I have not yet been able fully to determine ; but I strongly suspect it may, under particular circumstances, be made to crystallize."

Notwithstanding the unqualified assertion by which some of these facts, as well as those respecting strontian, are accompanied, it will appear that all of them were well known, and had been repeatedly described, before the appearance of Mr. Hume's paper, in French, German, and English.

1. The solution of sulphate of barytes in sulphuric acid, the crystallization of the solution, and its decomposition by water, are all stated in a letter from Morveau to Bergman : Journal de Physique, vol. xviii. p. 299, 1781.

2. These facts, with the additional one of the decomposition of the carbonate of barytes by sulphuric acid, are all described by Dr. Withering : Phil. Trans. 1784.

3. Excepting the crystallization of the solution of the sulphate of barytes, the same circumstances are related by Sage in the paper already quoted : Memoires de l'Academie, &c. 1788.

4. With the above exception, they are also detailed in the Journal de Physique, 1788 ; Sage's memoir having been also printed in that work, with some alterations.

5. These facts are all mentioned by Klaproth, in Crell's Annals, 1795.

6. The solubility of sulphate of barytes in sulphuric acid, and the decomposition of the solution by water, are stated by Kirwan : Mineralogy, vol. i. p. 136, 1794.

7. The experiments alluded to are all described with such perfect clearness by Klaproth, that I shall quote the passage from the English translation, vol. i. p. 234, printed in London in 1801, the year before that in which Mr. Hume's paper was published.

After describing the solution of strontian in sulphuric acid, as already quoted, he adds, " In like manner, 60 grains of witherite were combined with two ounces of strong sulphuric acid. A great effervescence ensued ; and, with the assistance of boiling heat, a complete solution as clear as water was likewise in this case produced. Some days after, the greatest part of this solution formed a crystalline mass of very tender fibres. This solution was also immediately decomposed by the admixture of water, and sulphate of barytes precipitated."

The pages of the *Annals of Philosophy*, the Philosophical Magazine, and the Medical and Physical Journal, exhibit ample evidence of Mr. Hume's proneness to complain without having been injured, and to attack under semblance of defence : in future he will probably be more cautious ; and, considering his situation with respect to the authors I have quoted, it will evince his prudence to refrain from charging those who may hereafter even unfairly question the priority of his remaining or future discoveries, with the conduct unmeritedly ascribed to two most respectable physicians, of " unjustly throwing the veil over his efforts, in order to display their own." *

Article VII.

Description of a sensible and convenient Method, which serves as a Magnetometer. By W. A. Lampadius.†

To procure an easy and convenient instrument to determine the weight of small quantities of matter, and to ascertain the smallest change in the weight of bodies under examination, I employed the following method, which serves likewise the purpose of a magnetometer, and may be employed for other hygroscopical and similar purposes ;—

* Vide Med. and Phys. Journal, vol. xxvi. p. 109.
† Translated from Schweigger's Neues Journal fur Chemie und Physik, x. 171. 1814.

I suspend by an oiled silk thread a moderately even glass rod 24 inches long. The point of suspension is eight inches from the left end of the rod, so that the other arm of the lever is 16 inches in length, and at the extremity of it is fixed a silver pointer to show the motion of the arm. To the shorter end of the lever is fixed a small leaden weight, secured from oxidation by being varnished, and from the extremity of the longer arm of the lever a small glass scale is suspended by silken threads.

The weight is so regulated that the pointer stands always at 0. (Pl. XXV. fig. 7.) By putting one grain of apothecaries' weight into the glass scale, the pointer sinks a little; and still farther on adding two, three, four, &c. grains. The positions of the pointer with each weight in the scale are marked on a circle against which the pointer plays. As the distance between each of these positions is $1\frac{1}{2}$ inch, I divide it into 100 parts, and thus I obtain a means of determining the change of weight amounting to $\frac{1}{100}$ part of a grain. To prevent the lever from being agitated backwards and forwards, it is made to play between two perpendicular glass rods placed at *a*. Suppose, now, that I have melted or heated a small piece of matter by means of a fire urged by oxygen gas, and that I wish to ascertain what change has taken place in its weight ; my instrument puts it in my power to do so. In order to determine the magnetic energy of a body, I take a piece of it weighing one grain = 100 of my divisions, and put it into the small glass scale. Then, by means of a screw, I apply a magnet capable of supporting one pound of iron, always in a direction exactly under the scale in which the substance to be examined is deposited. When the magnet comes within the sphere of attraction of the body, the pointer usually starts a little up, as, for example, to 95. I now cautiously push the magnet forward with the screw. At last the scale descends towards the magnet. I then observe the pointer again. Supposing it to stand at 155, I consider 55 as indicating the magnetic energy of the body ; so that in the present case this energy is a little more than equal to half the weight of the body. I repeat the experiment six or ten times, and never find more than $\frac{1}{100}$ part of difference.

In making this communication, I do not pretend to point out any thing new in the use of the lever, but only to make known a convenient application of what is already well understood. It will be understood, I suppose, that the glass rod employed must not be thicker than a common thermometer tube, and that the glass scale itself must be small and light.

Article VIII.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude, 51° 32' 40·3" North.   Longitude West in Time 6"$\frac{82}{100}$.

Oct. 30, Immersion 1 δ δ 12ʰ 58' 38·7" Mean Time at Hackney Wick.

Magnetical Observations.

1814.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Oct. 18	8ʰ 35'	24° 14'	06"	1ʰ 40'	24° 20'	56"		
Ditto 19	8 45	24 15	21	1 40	24 22	17		
Ditto 20	8 40	24 13	20	1 25	24 22	30		
Ditto 21	— —	— —	—	1 40	24 22	58		
Ditto 22	8 30	24 14	18	2 00	24 19	43	Not observed.	Not observed.
Ditto 23	8 35	24 13	25	1 45	24 21	31		
Ditto 24	8 50	24 14	06	1 50	24 20	07		
Ditto 25	8 45	24 12	40	1 40	24 21	24		
Ditto 26	8 50	24 15	10	1 45	24 21	32		
Ditto 27	8 30	24 14	47	1 40	24 24	18		
Ditto 28	8 45	24 17	52	1 40	24 21	02		
Ditto 29	8 35	24 17	06	1 40	24 19	46		
Ditto 30	8 40	24 16	10	1 30	24 23	36		
Ditto 31	8 35	24 15	27	1 45	24 20	22		

1814.

Mean of Observations in Oct.	Morning	at 8ʰ 39'	..... Variation	24° 14' 08"	} West.
	Noon	at 1 42	..... Ditto	24 21 45	
	Evening	at — —	..... Ditto	— — —	Not obs.
Ditto in Sept.	Morning	at 8 32	..... Ditto	24 14 33	} West.
	Noon	at 1 39	..... Ditto	24 23 17	
	Evening	at 6 19	..... Ditto	24 16 50	
Ditto in Aug.	Morning	at 8 30	..... Ditto	24 14 13	} West.
	Noon	at 1 39	..... Ditto	24 23 48	
	Evening	at 6 57	..... Ditto	24 16 31	
Ditto in July.	Morning	at 8 41	..... Ditto	24 13 29	} West.
	Noon	at 1 42	..... Ditto	24 23 44	
	Evening	at 6 58	..... Ditto	24 17 00	
Ditto in June.	Morning	at 8 44	..... Ditto	24 13 10	} West.
	Noon	at 1 30	..... Ditto	24 22 48	
	Evening	at 6 52	..... Ditto	24 16 29	
Ditto in May.	Morning	at 8 45	..... Ditto	24 13 12	} West.
	Noon	at 1 44	..... Ditto	24 22 13	
	Evening	at 6 38	..... Ditto	24 16 14	
Ditto in April.	Morning	at 8 45	..... Ditto	24 12 53	} West.
	Noon	at 1 48	..... Ditto	24 23 53	
	Evening	at 6 29	..... Ditto	24 15 30	
Ditto in March.	Morning	at 8 52	..... Ditto	24 14 29	} West.
	Noon	at 1 52	..... Ditto	24 23 08	
	Evening	at 6 11	..... Ditto	24 15 33	
Ditto in Feb.	Morning	at 8 47	..... Ditto	24 14 50	} West.
	Noon	at 1 52	..... Ditto	24 20 58	
	Evening	at — —	..... Ditto	— — —	Not obs.

Ditto in Jan.	Morning	at	8	52	.....	Ditto	24	15	05	West.
	Noon	at	1	53	.....	Ditto	24	19	03	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
1813.	Morning	at	8	53	.....	Ditto	24	17	39	West.
Ditto in Dec.	Noon	at	1	51	.....	Ditto	24	20	30	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Nov.	Morning	at	8	40	.....	Ditto	24	17	17	West.
	Noon	at	1	54	.....	Ditto	24	20	24	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Oct.	Morning	at	8	45	.....	Ditto	24	15	41	West.
	Noon	at	1	59	.....	Ditto	24	22	53	
	Evening	at	—	—	.....	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	.....	Ditto	24	15	46	West.
	Noon	at	2	02	.....	Ditto	24	22	32	
	Evening	at	6	03	.....	Ditto	24	16	04	
Ditto in Aug.	Morning	at	8	44	.....	Ditto	24	15	55	West.
	Noon	at	2	02	.....	Ditto	24	23	32	
	Evening	at	7	05	.....	Ditto	24	16	08	
Ditto in July.	Morning	at	8	37	.....	Ditto	24	14	32	West.
	Noon	at	1	50	.....	Ditto	24	23	04	
	Evening	at	7	08	.....	Ditto	24	16	43	
Ditto in June.	Morning	at	8	30	.....	Ditto	24	12	55	West.
	Noon	at	1	33	.....	Ditto	24	22	17	
	Evening	at	7	04	.....	Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	.....	Ditto	24	12	02	West.
	Noon	at	1	37	.....	Ditto	24	20	54	
	Evening	at	6	40	.....	Ditto	24	13	47	
Ditto in April.	Morning	at	8	31	.....	Ditto	24	09	18	West.
	Noon	at	0	59	.....	Ditto	24	21	12	
	Evening	at	5	46	.....	Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 1	8ʰ 40′	24° 13′ 58″	1ʰ 35′	24° 21′ 09″		
Ditto 2	8 50	24 15 38	1 40	24 20 52		
Ditto 3	8 45	24 16 25	— —	24 — —		
Ditto 4	8 35	24 19 40	1 45	24 26 12		
Ditto 5	8 45	24 16 02	— —	24 — —		
Ditto 6	8 45	24 15 27	1 40	24 20 08	Not observed.	Not observed.
Ditto 7	8 35	24 15 03	1 45	24 20 11		
Ditto 8	8 45	24 16 06	2 00	24 19 00		
Ditto 9	8 40	24 14 28	1 40	24 20 30		
Ditto 10	8 40	24 16 52	— —	— — —		
Ditto 11	8 40	24 16 13	1 50	24 20 40		
Ditto 12	— —	— — —	1 35	24 22 49		
Ditto 13	8 30	24 16 23	1 40	24 20 27		
Ditto 14	8 40	24 16 35	1 35	24 20 07		
Ditto 15	8 45	24 16 06	1 40	24 21 03		
Ditto 16	— —	— — —	1 45	24 20 23		
Ditto 17	8 30	24 15 30	— —	24 — —		

Nov. 8.—The needle at noon unsteady; and in the evening a storm of wind and rain, accompanied with thunder, from the south-west.

Nov. 15.—The needle unsteady, and followed by a hard gale of wind and rain from the south-west.

Nov. 16.—The wind at noon blew very hard and squally from
W. N. W. The weather clear. The needle vibrated at intervals
13′ 25″.

Rain fallen $\begin{Bmatrix} \text{Between noon of the 1st Oct.} \\ \text{Between noon of the 1st Nov.} \end{Bmatrix}$ 2·126 inches.

Comparison of the Variation in the Years 1813 *and* 1814.

		1813.			1814.			Difference.		
April	Morning	24°	09′	18″	24°	12′	53″	+	3′	25″
	Noon	24	21	12	24	23	53	+	2	41
	Evening	24	15	25	24	15	30	+	0	05
May	Morning	24	12	02	24	12	49	+	0	47
	Noon	24	20	54	24	22	13	+	1	19
	Evening	24	13	47	24	16	14	+	2	27
June	Morning	24	12	35	24	13	10	+	0	35
	Noon	24	22	17	24	22	48	+	0	31
	Evening	24	16	04	24	16	18	+	0	44
July	Morning	24	14	32	24	13	29	−	1	03
	Noon	24	23	04	24	23	44	+	0	40
	Evening	24	16	43	24	17	00	+	0	17
Aug.	Morning	24	15	55	24	14	13	−	1	42
	Noon	24	23	32	24	23	48	+	0	16
	Evening	24	16	08	24	16	31	+	0	23
Sept.	Morning	24	15	46	24	14	33	−	1	16
	Noon	24	22	32	24	23	17	+	0	45
	Evening	24	16	04	24	16	50	+	0	46
Oct.	Morning	24	15	41	24	14	08	−	1	33
	Noon	24	22	53	24	21	45	−	1	08
	Evening	—	—	—	—	—	—	—	—	—

Article IX.

Observations on Mr. Goring's new Solvent for Calculi. By Mr.
F. I. Armiger, Surgeon to the Eastern Dispensary.

SIR,   37 Camomile-street, Nov. 12, 1814.

HAVING in your Journal for the present month read a paper
by Mr. C. R. Goring, On a new Solvent for all sorts of Urinary
Concretions, I cannot resist offering the following remarks. It
is not my intention to consider the power of galvanic electricity
in decomposing concretions, but *to object to the manner* in which
that agent is recommended to be employed in such cases.

Mr. Goring must surely be unacquainted with practical surgery
to suppose that the introduction of an instrument *through the
urethra* is a greater evil than that of *making a puncture into the
urinary bladder.*

If that organ be opened at all under such circumstances, would
not the opening be made *large enough* to *extract the stone?*

Can it be conceived that a person with a puncture in the

bladder *would be able,* or, if able, *would have resolution,* to conduct such a process as that described by Mr. Goring.

Such a proceeding might be adopted in some living brute animal, if one were found with a calculus in the urinary bladder, for the sake of manifesting *the power and effects of galvanism :* but I cannot conceive that *any circumstance or condition can authorise such a practice on the living human body.**

I am, Sir, your obedient servant,

F. I. ARMIGER.

ARTICLE X.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London for the Year 1814. Part 1.

This volume contains the following papers :—

1. *A Synoptic Scale of Chemical Equivalents.* By William Hyde Wollaston, M.D. Sec. R.S.—A full account of this important instrument has been given in the 21st number of the *Annals,* to which therefore the reader is referred.

2. *Method of clearing Equations of Quadratic, Cubic, Quadrato-Cubic, and higher Surds.* By William Allman, M.D.—This method, which is very ingenious and simple (if such a term can be applied to the resolution of equations involving the higher powers), originated, it appears, with Dr. Mooney, Fellow of Trinity College, Dublin. It applies universally till we come to equations of the 11th power. Such methods are of considerable importance, by the tendency which they have to throw additional light upon the nature of equations ; though I doubt whether they would be found convenient enough for use in practical cases.

3. *Analysis of a New Species of Copper Ore.* By Thomas Thomson, M.D. F.R.S. L. and E.—This ore was brought from the Mysore by Dr. Heyne, where, from his account, it seems to exist in prodigious quantities, chiefly in nests in primitive rocks. This ore has a brown colour, a conchoidal fracture, is soft, sectile, and of the specific gravity 2·620. It is an anhydrous carbonate of copper, mixed with red oxide of iron. Its constituents were found as follows :—

* In justice to Mr. Goring, I think it proper to state that I received a letter from him, dated Nov. 5, requesting me not to publish his proposal. My opinion of the proposal was the same as that of Mr. Armiger, and indeed of every medical man, that it was quite impracticable. I published it because I was conscious that the proposal could do no harm, as it was not susceptible of being tried. I conceived that Mr. Goring himself would immediately be convinced of its impracticability when he saw it in print. The small space that it occupied in the *Annals* rendered it in every respect innocent.

Carbonic acid	16·70
Peroxide of copper	60·75
Peroxide of iron	19·50
Silica	2·10
Loss	0·95
	100·00

The silica is accidental. The carbonic acid is united to the copper, constituting common carbonate of copper without water, which is composed of one atom carbonic acid and one atom peroxide of copper. The red oxide of iron is only mechanically mixed.

4. *The Bakerian Lecture : on some New Electro-Chemical Phenomena.* By William Thomas Brande, Esq. F.R.S. Prof. Chem. R.I.—Mr. Cuthbertson observed that when the flame of a candle is introduced between a negative and positive ball, the negative ball becomes hotter than the positive. Mr. Erman has shown that certain substances are unipolar with regard to the electricity of the voltaic pile. The insulated flames of wax, oil, spirit of wine, and hydrogen gas, only conduct positive electricity ; while the flame of phosphorus conducts only negative electricity. It occurred to Mr. Brande that these facts admitted of another explanation. It is known that some chemical bodies are naturally positive, others naturally negative. The positive of course would be attracted by the negative ball, and the negative by the positive. To determine the validity of this conjecture, he made a set of experiments on various bodies, the results of which are related in the present lecture.

The apparatus employed consisted of two insulated brass balls, the distance of which from each other could be varied at pleasure. One of them was attached to the positive, the other to the negative, prime conductor of an electrical machine ; and care was taken that this machine should be in weak action. The following were the results obtained : When olefiant gas was burnt between the balls, the flame was attracted towards the negative side. The flame of sulphureted hydrogen was slightly attracted to the negative side ; the sulphurous acid vapour formed was attracted towards the positive ball. A small flame of phosphureted hydrogen was rather attracted to the positive ball ; a large flame of this substance was equally attracted by both balls. The flame of arsenicated hydrogen was attracted by the negative ball ; the fumes of white arsenic were slightly drawn towards the positive ball. The flame of hydrogen was weakly attracted towards the negative ball, but not very decidedly so. A large flame of carbonic oxide was attracted by the positive ball. When sulphur is burnt, the vapour is attracted by the positive ball. The direction of the flame could not be determined. The flame of sulphuret of carbon was attracted to the negative ball. The flame of phosphorus and its vapour were

attracted to the positive ball. A stream of muriatic acid gas was attracted towards the positive ball. This was the case likewise with nitrous gas. Potassium in combustion and its fumes were drawn to the negative ball. The results with ammonia were uncertain. Vapour of benzoic acid was attracted by the positive ball. But when benzoin was burnt, the flame and smoke took the opposite direction. The charcoal emitted by camphor in combustion is attracted by the negative ball. Resinous bodies exhibit the same appearances as camphor; and amber the same as benzoin.

These experiments possess considerable value, by furnishing additional facts in support of a theory which promises to new model both the sciences of electricity and chemistry. I cannot at present enter into any discussion respecting this theory without deviating from my general plan; but an opportunity will soon occur of laying an abstract of it before the reader, and of examining how far it is supported by facts, and consistent with the known laws of chemistry and electricity.

5. *An Account of some New Experiments on the Fluoric Compounds, with some Observations on other Objects of Chemical Inquiry.* By Sir H. Davy, LL.D. F.R.S. V.P.R.I.—It has been repeatedly mentioned in the *Annals* that Sir H. Davy has adopted an hypothesis originally suggested by M. Ampere; that fluoric acid is a compound of hydrogen and an unknown supporter, to which he has given the name of *fluorine*; that silicated fluoric acid is a compound of fluorine and the basis of silica; and fluoboric acid, of fluorine and boron. The object of the first part of this paper is to give an account of some unsuccessful attempts to obtain fluorine in a separate state.

Fluate of lime, while moist, is decomposed by ammonia, and a white powder separated; but dry fluate of lead may be fused in ammonia without undergoing any change. When moist silicated fluate of ammonia and fluo-borate of ammonia are heated in chlorine, silica and boracic acid are given out; but when these bodies are dry, muriate of ammonia is formed, and silicated fluoric acid and fluoboric acid disengaged. Charcoal, ignited by means of the galvanic battery in fluoboric or silicated fluoric acids, produces no decomposition. Liquid fluoric acid passed over charcoal, heated to whiteness in a platinum tube, was not decomposed.

A hundred parts of pure white Derbyshire spar, when decomposed by sulphuric acid, yield 175·2 of sulphate of lime. Now 175·2 of sulphate of lime contain 73·58 lime. Therefore if we adopt the common opinion that Derbyshire spar is a fluate of lime, its composition will be

$$
\begin{array}{llll}
\text{Fluoric acid} & \dots\dots\dots\dots & 26\cdot42 & \dots\dots & 100 \\
\text{Lime} & \dots\dots\dots\dots\dots & 73\cdot58 & \dots\dots & 278\cdot49 \\
\hline
& & 100\cdot00 &
\end{array}
$$

And if this salt, according to the analogy of the other salts of

lime, be composed of one atom lime and one atom acid, then an atom of fluoric acid will weigh 1·299. It appears from this result that the atom of fluoric acid is the lightest of all the acids, and that it cannot contain more than a single atom of oxygen. This is certainly against the common opinion respecting the composition of fluoric acid ; as we are not acquainted with any other acid that contains so little oxygen as a single atom.

But according to Sir H. Davy's view of the matter, Derbyshire spar is a compound of fluorine and calcium. Now 73·58 lime contain 20·32 oxygen; so that according to this view the spar is composed of

$$
\begin{array}{lll}
\text{Fluorine} \dotfill & 46\cdot74 & \dotfill \ 100 \\
\text{Calcium} \dotfill & 53\cdot26 & \dotfill \ 113\cdot95 \\
\cline{2-2}
& 100\cdot00 &
\end{array}
$$

If we suppose this a compound of an atom of calcium and an atom of fluorine, an atom of fluorine will weigh 2·299.

Davy has given some other analyses of the fluates, which it may be worth while to state : 22 grains of fused subcarbonate of potash (containing 31 per cent. of carbonic acid,) formed 18·15 grains of dry fluate of potash. These 18·15 grains, treated with sulphuric acid, gave 38·5 of bisulphate of potash. 100 parts of solution of ammonia, of the specific gravity 0·9162, require for saturation the quantity of fluoric acid contained in 32 grains of fluate of potash.

The second part of this paper contains an account of a number of unsuccessful attempts to obtain the base of silica in a separate state. When silica is heated with potassium it is decomposed ; but the base cannot be separated from the alkali by means of water, as it decomposes that liquid, and is converted again into silica. But Sir H. Davy has rendered it very probable that the base of this earth is not a metal, but a substance analogous to boron in its properties.

The last part of this paper consists of a statement of the reasons why chlorine should be considered as an undecompounded substance, and muriatic acid as a compound of chlorine and hydrogen. This part of the paper is remarkably well drawn up, and is written in a style of clearness and elegant simplicity, so much to be desired in philosophical writings.

6. *Some Experiments and Observations on a New Substance, which becomes a Violet-coloured Gas by Heat.* By Sir Humphry Davy, Knt. LL.D. F.R.S.—This substance, now familiar I presume to most British chemists, was discovered about three years ago by M. Courtois, a saltpetre-maker in Paris. It was first examined by Clement and Desormes, and afterwards by Gay-Lussac. The present paper contains Sir H. Davy's experiments on it.

This substance, called *iodine* from the violet-coloured vapour which it forms when heated, is obtained from kelp, or spent soapers' leys ; and French kelp yields a much greater proportion of

6

it than British kelp.   It is a solid substance, having the appearance of plumbago; but it is very volatile, and gives out a peculiar odour, and it is proper to know that its properties are decidedly poisonous. The properties of iodine stated in this paper are the following :—

It is a substance analogous to oxygen and chlorine, and could not be decomposed by any methods which the author could devise. It is partially soluble in water, and soluble likewise in alcohol and ether.   When its solution is mixed with nitrate of silver, a lemon-yellow precipitate falls, which melts in a low red heat, and becomes red.   It is rapidly decomposed by caustic potash, oxide of silver being separated.   When iodine is passed over red-hot silver, the same substance is formed.

When iodine is passed over hot potassium, that metal burns with a pale blue flame, no gas is given out, but a white substance is formed, soluble in water, and fusible in a red heat.   It has an acrid taste, and when treated with sulphuric acid yields iodine.

Iodine absorbs chlorine, and forms a volatile solid substance of a yellow colour, soluble in water, and forming an acid to which the name of *chlorionic* acid has been given.

Iodine, when heated in oxygen gas, or brought in contact with red-hot hyperoxymuriate of potash, undergoes no change.

It combines readily with metals, without any violent action. With tin it forms a deep orange substance; with iron, a brownish red ; with lead, a bright orange; with mercury, a bright crimson; and with zinc, a white compound.   The iode of iron was not decomposed by ammoniacal gas, but combines with it.   The iode of tin has the properties of an acid, and is called stanniodic acid.

Iodine combines very rapidly with phosphorus, producing heat without light.   When the iodine is in excess, a red volatile solid is produced ; when the phosphorus is in excess, the compound is more fixed.   During this combination an acid gas is evolved, at first taken for muriatic acid, but possessing different properties.   It is absorbed by water and by mercury.   This acid is called *hydrionic acid*.*   Abundance of this acid is exhaled when the iode of phosphorus is heated in water, and phosphorous acid remains behind. Davy considers this acid as a compound of iodine and hydrogen.

Iodine combines readily with potash and soda, and forms with each two saline compounds ; the first, composed of oxygen, iodine, and the alkaline base, is analogous to the hyperoxymuriate of potash ; the second is more soluble, and is a compound of iodine and the metallic base of the alkalies.   Similar compounds are obtained with barytes, and probably all the alkaline earths.

When iodine in vapour is passed over dry red-hot potash, oxygen is expelled.

Iodine is expelled from its compounds by chlorine, but in general

* Davy calls it *hydroionic* acid ; but it is better to avoid the hiatus of so many vowels.

it expels oxygen from its combinations, though it is driven off from phosphorus and sulphur by oxygen.

Iodine, when put into ammonia, forms a black powder, which detonates when dry. According to Davy, it is a compound of iodine and azote.

Four grains of hydrate of potash are saturated by $6\frac{1}{4}$ grains of iodine. One grain of hydrate of soda requires $2\cdot1$ grains of iodine. The first of these estimates gives the weight of an atom of iodine $11\cdot160$, the second $10\cdot680$. My own experiments raise it as high as $12\cdot5$.

Mercury absorbs nearly three fourths of its weight of iodine. This paper terminates with some observations on nomenclature. But as they have been stated already in a former number of the *Annals*, I consider it unnecessary to repeat them here.

7. *An Account of a Family having Hands and Feet with Supernumerary Fingers and Toes.* By Anthony Carlisle, Esq. F.R.S.—This singular structure exists in the family of Zerah Colburn, the remarkable American calculating boy, who was exhibited two winters ago in London. The family belongs to the state of Vermont, in North America. There are five fingers upon each hand and six toes on each foot. The supernumerary finger is upon the outside of the little finger of each hand, and there is a metacarpal bone with all the necessary appendages to it. The finger is regularly formed. The same remark applies to the feet. This peculiarity has been propagated in the family for at least four generations. A woman of the name of Kendall had it; she married Mr. Green, who was naturally formed; they had eleven children, all of them with five fingers and six toes. One of the daughters of this marriage married David Colburn, who was naturally formed; they had four children, three with five fingers and six toes on each hand and foot, one with one hand and foot naturally formed, the others with the monstrosity. Abiah Colburn, a son by this marriage, having the peculiarity, married a woman naturally formed; they have eight children, three naturally formed, and five with the peculiarity; one of these five is the calculating boy.

8. *Experiments and Observations on the Influence of the Nerves of the Eighth Pair on the Secretions of the Stomach.* By B. C. Brodie, Esq. F.R.S.—An opinion had been advanced by Sir Everard Home that the various animal secretions are dependant on the influence of the nervous system; and this opinion seemed strengthened by some physiological experiments of Mr. Brodie. The object of the present experiments is to show that the mucous secretion of the stomach produced by the action of arsenic on that organ is prevented by cutting the nerves which supply the stomach. Four cases are related of dogs poisoned by arsenic, in which the nerves of the stomach were cut, and the mucous secretion did not take place.

9. *On a Fossil Human Skeleton from Guadaloupe.* By Charles König, Esq. F.R.S.—This fossil excited considerable interest in

London, as being the only one of the kind hitherto observed. Mr. Konig's description, as far as I am able to judge, is very precise, and sufficiently minute. This skeleton was found in that part of Guadaloupe called Basse Terre, in a kind of lime-stone rock extending along the shore about a mile in length, and within high-water mark. The block was raised by the French, and destined for Paris. Sir Alexander Cochrane altered its destiny, and the Lords of the Admiralty presented it to the British Museum. This rock is of lime-stone, and varies in appearance in different parts. In general it resembles sand-stone, and has a yellowish-grey colour; but in certain places it more nearly resembles compact lime-stone. It consists of yellow grains mixed with others of a flesh-red colour. The red grains are the detritus of a millepore, and probably the *millepora miniacea* of Pallas. It contains also shells; one of these is a helix, approaching to the *helix acuta* of Martini; another is a variety of the *turbo pica* of Linnæus. The skeleton wants the head and neck. The seven true ribs of the left side are complete; the upper part of those of the right is likewise found on the left side, and probably the sternum is concealed in the stone. A portion of the bones of both arms is lost. The metacarpal bones of the left hand, and part of those of the fingers, remain. Vestiges of the vertebræ are visible all along. The superior part of the os sacrum is distinct. The bones of the right thigh and leg are in good preservation; those of the left are partly lost. All the bones are broken in various directions, and filled with lime-stone; the swelling of which within the bones probably occasioned the fractures. These bones are soft, and still retain animal matter, besides the phosphate of lime, as is evident from the smell they emit when burnt.

10. *A New Method of deducing a first Approximation to the Orbit of a Comet from three Geocentric Observations.* By James Ivory, A.M.—Newton was the first person that proposed a method for determining the orbits of comets; but his method, for a reason afterwards pointed out by Boscovich, did not answer. Much labour has been bestowed on this difficult subject by the Continental philosophers. The methods of Boscovich, Laplace, and Legendre, Mr. Ivory considers as the only ones of practical utility; but his own method he gives as much simpler and easier than theirs, and reduced to as much simplicity as the nature of the case will admit. But for an account of this method we must refer to the volume of the Transactions itself, as any abridged statement of it could not be attended with any utility.

11. *On the Affections of Light transmitted through Crystallized Bodies.* By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin.—A pretty full account of this interesting paper having been already given in the *Annals of Philosophy,* I shall be the shorter in this place. The paper is divided into five parts. 1. The first part treats of the *Polarizing Power of the Agate.* Light passing through an agate cut in a plane perpendicular to its laminæ is polarized. The pencil of polarized rays is surrounded by a large

mass of nebulous light. This the author conceived to be an imperfect image, and drew as a consequence that the agate is in a state of approach to that particular kind of crystallization which affords double images. His subsequent experiments on depolarization appear to have demonstrated the truth of this opinion. 2. *On the Structure of the Agate as connected with its Optical Properties.* The agate is composed of layers (usually concentric) partly transparent and partly opake. The opake layers appear milk-white by reflected light, but brown by transmitted light. The transparent layers are formed of waving lines, in some specimens very fine, in others coarser. When the incident ray is parallel to these layers it is transmitted in greater abundance than when oblique; and when it is transmitted in one oblique direction more of it passes than when in another. These differences obviously depend upon something peculiar to the structure of the agate. 3. *On the peculiar Colours exhibited by the Agate.* The author observed the existence of a coloured image on each side of the colourless image, and polarized in the same manner. This coloured image is equally distinct in every position of the agate; it is alike produced by polarized and unpolarized light, and suffers no change when examined by a plate of agate or by a doubly refracting crystal. 4. *On the Depolarization of Light.* Almost all transparent bodies possess in two positions the property of depolarizing light, while in other two positions they do not affect it. The first of these positions the author calls the *depolarizing axes*; the second, the *neutral axes.* 5. *On the Eliptical-Coloured Rings produced by depolarizing Crystals.* For this curious subject we must refer to the paper itself, as it cannot be intelligibly explained without figures.

12. *On the Polarization of Light by oblique Transmission through all Bodies, whether Crystallized or Uncrystallized.* By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin.—When Dr. Brewster wrote this paper he was not aware that the important discovery which it communicates had been already made by Malus. The little intercourse between Great Britain and France, together with the death of Malus, having prevented his recent discoveries from being known in this country, except to one or two individuals who had made optics their favourite pursuit: so far therefore as the merit of discovery goes, Dr. Brewster stands upon the same footing as Malus; though the priority of the latter no doubt deprives him of a great part of that eclat to which he would have otherwise been entitled. When light is transmitted obliquely through bundles of plates of transparent bodies, as glass, it is polarized, according to a certain law which depends upon the angle of incidence of the light and the number of plates through which it passes. Let n, n', represent the number of plates in any two parcels, and φ, φ', the angles at which the pencil is polarized; we have $n : n' :: \text{cotang. } \varphi : \text{cotang. } \varphi'$, and $n \times \text{tang. } \varphi = n' \times \text{tang. } \varphi'$.

Hence it follows that the number of plates in any parcel multiplied by the tangent of the angle at which it polarizes light is a

constant quantity. Dr. Brewster found the constant quantity for crown glass to be 41·84. If we divide this constant quantity by the number of plates, the quotient will be the natural tangent of the angle at which that number will polarize a pencil of light. This law has enabled the author to construct a table exhibiting the angles of incidence at which a ray of light is polarized by any number of plates from one to 8,640,000.

Dr. Brewster likewise observed, as Malus had done before him, that the ray of light was polarized by transmission in the opposite way from what it was by reflection, and that the whole of the transmitted light being polarized will pass through the glass plates without any of it being lost.

13. *Farther Experiments on the Light of the Cassegrainian Telescope compared with that of the Gregorian.* By Captain Henry Kater, Brigade Major.—In this paper Captain Kater relates another experiment which he made on the relative intensity of the light of these two telescopes. It confirmed his preceding results. It seems to follow from these experiments that the light of the Cassegrainian telescope is to that of the Gregorian as 234 to 100. In an appendix to the paper Capt. Kater relates a number of experiments made to determine the relative intensity of light from a concave mirror at equal distances from the focus within and without. From these experiments it would appear that the intensity of the light within the focus is to that without very nearly in the ratio of two to one. The mean of all the experiments give the ratio 1000 to 503·68.

14. *Astronomical Observations relating to the Siderial Part of the Heavens, and its Connection with the Nebulous Part, arranged for the Purpose of a Critical Examination.* By William Herschel, LL. D. F. R. S.—Dr. Herschel, with whose ingenuity and originality of ideas most of our readers are sufficiently acquainted, has been occupied for many years in studying the structure of the siderial heavens. He has at last started an opinion respecting the formation of the stars, which he has supported in several papers with much skill and address. The numerous nebulosities which fill the heavens, being gradually condensed by gravitation, are converted into stars. Stars ready formed occasionally attract nebulosities, and increase in size. Neighbouring stars by mutual attraction gradually approach other, and constitute globular clusters. Such is a short outline of the hypothesis. The present paper consists in a kind of arrangement of facts in support of the probability of the hypothesis. It is not very susceptible of abridgment, and indeed requires the knowledge of several of Dr. Herschel's previous papers to be completely understood. These reasons prevent me from attempting to lay a view of it before the reader. There is another reason also which has some weight with me. I consider it as treating of a subject beyond the reach of our limited means of investigation upon this earth; and as the objects of investigation which find a place in the *Annals of Philosophy* are of a very different kind, I conceive the present paper to lie in some measure beyond our province.

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Society resumed its meetings on Thursday, the 10th of November. A paper by Dr. Brewster on the Optical Properties of Prince Rupert's Drops, was read. Dr. Brewster had found that the glass tears known by the name of Prince Rupert's drops, and formed by dropping melted glass into water, has the property of depolarizing light. On examining them farther he found that each had a number of cleavages, consisting of lines diverging from a point near the bottom of the drop. He observed a second cleavage parallel to the axis ; and a third, but not so well defined, oblique to the axis. In short, the glass had a triple cleavage, and was crystallized. He found the specific gravity of these tears 3·264, and that of annealed glass 3·278. On slowly heating one of these tears to redness, its specific gravity became 3·278, and it lost the property of depolarizing light.

On Thursday, the 17th of November, a paper by Dr. Roget was read, describing an arithmetical instrument for involution and evolution. The only useful arithmetical instruments at present in use are the common sliding rules, by means of which multiplication and division are performed mechanically by means of a line logarithmically divided. Dr. Roget's machine is likewise a sliding rule, in which a line of logarithms is so placed as to point out the different powers or roots of any number ; but it would be impossible to make the plan intelligible without figures.

LINNÆAN SOCIETY.

The Society resumed its meetings on Tuesday, the 1st of November.

A paper by Mr. Sowerby was read on a fossil shell which occurs in chalk, very frequently in the flint nodules. Fragments of it had been observed by Cuvier and Brogniart in the chalk near Paris, and from their fibrous texture they were led to consider them as fragments of pinnæ ; but from their thickness (near half an inch) they concluded that the shell must have been of enormous size. Mr. Sowerby got specimens of the fossil from various quarters of the chalk country in the south of England. He ascertained, by comparing these specimens with each other, that it was a bivalve shell, having a hinge of a peculiar structure, and constituting a genus apart. To this genus he has given the name of *inoceramus* ; and the most common species he calls inoceramus Cuvierii.

At the same meeting, three papers by Mr. Rafanesque Smaltz were read. The first was a description of a new species of fish, which he observed during one of his voyages across the Atlantic. It

was a species of *echineis*. The second paper contained a description of a new species of tuber or truffle, which vegetates in Sicily. The third paper contained a description of two new genera of crustaceous animals found in the Sicilian seas. Of the first genus, *heterilos*, he described only one species, the *punctatus*, which lives about seven miles from the Sicilian coast, and is never seen on the shore. The species of the second genus, *hyamonus*, described, was found attached to a shark, and appears to live by suction of that fish.

On Tuesday, the 15th of November, a paper by Mr. Sowerby was read, on a genus of fossil shells not yet properly arranged or described by conchologists. The shell is a bivalve, with a triangular opening, and occurs in the liass lime-stone near Bristol, and likewise in flint in chalk. It is figured in the Encyclopedie Methodique, and likewise by Parkinson, who considers it provisionally as a pinna. Mr. Sowerby considers it as constituting a new genus, to which he has given the name of *plagiostoma*, and he describes three species.

At the same meeting, part of a Latin paper by M. Vieillot was read, in which the author gave a new elementary arrangement of birds. It is obviously impossible to form any conception of the merit of such an arrangement merely from hearing it read.

ROYAL IRISH ACADEMY.

A paper was read at a meeting of the Royal Irish Academy in May last, by Dr. Brinkley, Andrews Professor of Astronomy in the University of Dublin, containing an account of observations made at the Observatory of Trinity College, Dublin, with an astronomical circle eight feet in diameter, which appear to point at a parallax in certain fixed stars.

The results of the observations, continued for several years, uniformly tend to point out that the annual parallax of α Lyræ is about $2''$, of α Aquilæ $5\frac{1}{2}''$, of Arcturus $2\cdot2''$, and of α Cygni $2\cdot1''$. The author also notices indications of parallax in other stars, but reserves these for further examination. He mentions that till M. Piazzi, of Palermo, a few years since recalled the attention of astronomers to this subject, it had been generally conceived that the parallax of every fixed star was insensible.

The observations of Dr. Brinkley and of M. Piazzi, however, only accord nearly in two instances, viz. with respect to α Lyræ and Arcturus: with respect to α Aquilæ, which appears to Dr. Brinkley to have so sensible a parallax, M. Piazzi found no indication of parallax. This discordance, and the circumstance that his conclusions have not hitherto been confirmed by other astronomers, induced for some time much diffidence on the part of the author as to his results.

In this paper he enters into some account of the instrument, and gives such remarks respecting the observation as he hopes may tend to establish the same confidence in the minds of others as he now

himself feels respecting the important conclusions which he deduces from his observations.

Another paper by Dr. Brinkley was read at the same time, containing analytical investigations respecting astronomical refractions, and the application thereof to the formation of convenient tables, together with the results of observations of circumpolar stars, tending to illustrate the theory of refractions.

The author deduces, by a very short method, from the common principle of the constant ratio of the sine of incidence and refraction, the same fluxional expression that Laplace has deduced (Mec. Celest. tom. iv. p. 244). The approximate integration of the fluxional expression is obtained by a method which affords a result showing the effect of the spherical form of the atmosphere. The formula derived consists of two parts : one showing the refraction (ρ) that would take place were the surface of the earth a plane ; the other the effect due to the spherical form. The latter at 80° zenith distance amounts only to about 14″, and at 40° zenith distance is insensible.

Let θ = the zenith distance ; m : 1 the ratio of the sine of incidence to the sine of refraction in air of the density of that at the surface ; a = radius of the earth, and l = height of an uniform atmosphere. Then

$$\text{Refraction} = \rho - \frac{(m-1)\, l \tan.\theta}{a \cos.^2\theta \sin. 1''} + \frac{5\,(m-1)\, l^2 \tan.^3\theta}{2\,a^2 \cos.^2\theta \sin. 1''} \text{ nearly.}$$

It is shown that as far as 80° zenith distance this formula cannot err half a second, whatever be the law of variation of density in the atmosphere. The third term is insensible at 74°, and at 80° amounts only to $1\frac{1}{2}''$. At 40° zenith distance both the second and third are insensible. When θ is greater than about 80°, a knowledge of the law of variation of density is necessary for the integrations ; but as the approximate formula as far as about 74° is independent of the law of variation of density, it follows that whatever law of variation of density be assumed, the same conclusion ought to be deduced as far as about 74°. This is shown by direct investigation by assuming different laws of variation of density which, besides affording some conclusions useful in our inquiries on this subject, may be considered interesting.

The author then proceeds to deduce a formula convenient for computation and for tables. In this formula he substitutes the value of m as deduced from the experiments of Biot and Arago on the refractive force of air, and from the experiments of Dalton and Gay-Lussac on the change of density by the variation of temperature. In this manner a formula for refraction is deduced entirely independent in astronomical observations ; by which (barom. 29·60 and Fahrenheit's therm. 50) the refraction at 45° = 57·6″, at 74° = 198·6″.

The author gives his reasons for thinking that greater accuracy

may be expected from experiments on the refractive force and variation of density of air than from observations of circumpolar stars, even with the best instruments. It is still a question, as far as observations of circumpolar stars are concerned, whether at the above mean height of the barometer and thermometer, the refraction be 57″, according to Bradley, or 57·5″, according to the French tables; or whether, according to other astronomers, it may not exceed 58″. But the French tables agreeing so nearly with the above conclusions, the author conceives that as far as 74° they ought to be generally adopted. It is of considerable importance that all astronomers should use the same refractions, and these, by their close coincidence with what may be derived as above from the direct experiments on air, seem entitled to the preference. At 74° zenith distance (bar. 29·60, and Fahr. therm. 50) the French tables give refraction = 198·2″.

Dr. Brinkley gives two tables of refraction derived from the formula by which the same refraction as far as 74° is deduced as by the French tables. These tables are very convenient for use, equally so as the French tables; but they are principally given to afford a convenient check in the use of the French tables.

Below 80° a knowledge of the law of variation of density in the atmosphere is absolutely necessary for forming a table of refraction ; but if even this were known little advantage would thereby be gained in the nicer researches of astronomy, as within 10° or 12° of the horizon the quantity of refraction varies so much, the height of the barometer and thermometer being the same, as to render observations made with the best instruments of little value.

This paper concludes with the results of observations of circumpolar stars, and remarks thereon.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute of France during the Year 1813.

(*Concluded from* p. 391.)

VEGETABLE PHYSIOLOGY AND BOTANY.

M. Henri Cassini, son of one of our associates, a considerable set of observations by whom we announced last year on the great family of plants known by the name of *compound, syngenesiæ, synatheriæ,* struck with the principle explained by natural philosophers, that a classification to give a just idea of bodies ought to be founded on the whole of their characters, has this year examined the stamina of the same family, and has discovered several facts unknown to botanists. Nobody, for example, had observed the articulation which divides the filament in the neighbourhood of the anthera, a character which M. de Cassini has found much more

constant than the union of the antheræ with each other. He pro-
mises us immediately his observations on the corolla, the ovarium,
the pericarpium, and the seed ; and as there can be no doubt that
he has bestowed the same attention on these researches as upon
those which he has already communicated, no family of plants will
be better known. Botany has every thing to expect from so skilful
an observer, when, after having studied a family so natural that it
may be almost considered as constituting only one great genus, he
shall exercise his sagacity on those equivocal families the varying
characters of which render their limits uncertain.

Vegetable physiology, like all the other sciences, presents us with
difficult questions, of which nature furnishes us with no evident
solution, and which will long constitute the subjects of learned
discussions.

Such, among others, is that respecting the existence of sexes in
the plants known by the name of *cryptogamia.* Many botanists,
discouraged by the difficulty of discovering organs in them, have
formed the opinion that these vegetables are destitute of sexes, and
are propagated by bulbs or simple buds, like certain animals, such
as the polypi, in which there can be no doubt that reproduction
takes place in this manner. Others, on the contrary, struck with
the complicated apparatus for reproduction in the fungi, musci, &c.
cannot believe that a mode of production so simple as by bulbs
should render necessary so various and numerous organs. They have
endeavoured in consequence to discover stamina, pollen, pistils,
seeds, embrios, and all the agents of fecundation so well known in
ordinary plants. But as there is no analogy of form, though they
agree as to the principle, they differ in its application. What one
takes for pollen another considers as seeds, and the reverse ; so that
the *sexualists* (as they are called) have almost as many disputes with
each other as with the *agamists,* their common adversaries.

We have already in our former reports given an account of several
of these discussions. They have been renewed this year in conse-
quence of a laborious work of M. Desvaux on the family of lycopo-
diums. It is known that these plants (recently separated from the
other mosses by botanists) have a very combustible yellowish powder
in small capsules, well known by the name of *powder of lycopo-
dium,* and often employed for various purposes. Its resemblance to
the powder of the antheræ has induced M. de Beauvois to consider
it as true pollen. But according to some observers it does not burst
in water like pollen ; and all acknowledge that when sown in the
ground it grows, and produces lycopodiums. The first property not
being essential, M. de Beauvois ascribes the second to small glo-
bules, which he has observed in this powder, and which he con-
siders as small bulbs or buds ; so that according to him it is not the
yellow powder which grows, but some of those buds which it is
impossible to separate from it. As to the true seeds which the
pollen is destined to fecundate, M. de Beauvois finds them in other

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capsules, placed sometimes within, sometimes below the former, and containing only small, round, transparent grains, larger than those which constitute the yellow powder; but these particular capsules have not been found in more than one-third of the species of lycopodiums, and vain attempts have been made to discover them in the remainder.

M. Desvaux agrees nearly with M. de Beauvois as to the facts, but disputes the consequences drawn from them. He sees in the yellow powder nothing but bulbs and buds, or as he calls them *propagula*, which do not stand in need of fecundation in order to germinate. The other grains observed by M. de Beauvois, to judge from their small number, their transparence, and their varied and irregular figure, are merely abortive propagula.

M. de Beauvois replies by showing that all the definitions of seeds given by the most skilful botanists are applicable to these globules; and setting out from the principle that the existence of seeds supposes that of a female gender, and that the existence of one sex supposes the other, he maintains his original opinions.

His adversary replies that a nominal definition formed from received ideas cannot decide a dispute in which these very ideas are called in question, and that the visible characters of structure observed in all seeds are far from being verified here on account of the smallness of the object.

We see that the discussion begins to become metaphysical. The only mode of deciding the point would be to produce the fecundation of what is considered as the pistil by means of what is considered as the pollen. But who could flatter himself with being able to make upon organs so delicate an experiment, which has so fully demonstrated the existence of sexes in ordinary plants.

M. Desvaux has given likewise a methodical distribution of all the known lycopodiums, adding some subdivisions to those established by M. de Beauvois in a preceding memoir on the same family, and taking for his principal basis the existence of two sorts of capsules, and the division of the capsules into cells more or less numerous.

M. de Candolle, Correspondent of the Class, and Professor at Montpellier, has made us acquainted with parasite mushrooms of a new kind, which he names *rhizoctones*, or *death of the roots*, because they attach themselves to the roots of plants, and occasion their rapid death. M. Persoon had united under the name of *sclerotium* the mushrooms which are fleshy internally, like the *truffle*, but destitute of those veins which give a marbled appearance to the flesh of the truffle. Hedwig had separated from them the *erisiphes*, which live on the surface of leaves; but in those which remain there are still a sufficient number of characters to constitute two genera; one genus, which is not essentially parasite, and grows in dung and putrefying plants, has neither fibres nor roots at its surface; the other (constituting the *rhizoctones*) emits simple

filaments or branched ones, lives upon the roots of living plants, attacks them externally, and destroys them by absorbing their nourishment. These plants multiply with rapidity, by means of their filaments, which spread from one plant to another, and occasion contagious diseases, from which several of our cultivators have suffered. Only one species is well known which produces the disease, too famous in Gatinois, called *death of saffron.* Another species, which M. de Candolle describes for the first time, commits its ravages on lucern. Its filaments, of a fine lake colour, firmly embrace the roots of the lucern. The plants attacked droop, become yellow, and speedily die ; and as the mushroom propagates itself in radii, we speedily see in fields of lucern circular spots quite withered. The author advises to dig pits round the infected spots sufficiently deep to prevent the red filaments from crossing them, taking care to throw the earth from the pits into the circle, that the evil may not be increased in attempting to remedy it.

One of the greatest difficulties in botany consists in accurately fixing the limits of the species, and in not considering as species the varieties produced by the soil and the climate ; and the best means of avoiding these mistakes is not to admit as specific characters those circumstances belonging to the organization whose mutability has been ascertained. M. Desvaux, having applied this method to the roses, and having observed that several of the pretended species of this genus differ only in characters which frequently vary in the same individual, has succeeded in greatly reducing the nominal species of this plant. He has shown, for example, that the common wild rose (*rosa canina*) exhibits 21 varieties, all of which admit of being described, but which pass insensibly from the one to the other : 13 of these varieties have been improperly elevated to the rank of species by certain authors. Six other pretended species are likewise deprived of that rank, and brought under the *rosa alpina,* five under the *hedge rose,* &c. The same severity introduced into the whole of natural history would simplify and elucidate it very much. But for that purpose it would be necessary for naturalists to employ themselves in critical researches, and to renounce the vain honour of increasing without end the number of species. In the present state of the science there would certainly be more trouble, more utility, and more glory, in diminishing that list.

M. Delille, Member of the Institute of Egypt, has read to the Class an interesting history of the wild and cultivated plants in that famous country. He intends it as a part of the great general work on Egypt, to which so much talent concurs, and which is publishing with a magnificence corresponding to the greatness of an enterprize of which it will be the most durable monument. The author distinguishes the plants peculiar to Egypt from those brought by the inundations of the Nile and the winds of the desert, and from those common to it and the neighbouring countries. He fixes the

limits assigned to each species in that long and narrow valley by the latitudes, and the greater or smaller degree of saltness or sandiness of the soil. He makes us acquainted with the variations produced by each soil on the plants which vegetate in several; and he states with care the species that are cultivated, and the attention which each requires in consequence of the peculiar nature of that country, which is perhaps unique in its kind on the surface of the earth.

We regret much that a work consisting essentially in details does not admit of a sufficiently short analysis to enable us to introduce it into the narrow limits prescribed to us.

M. de Candolle has published an Elementary Theory of Botany, in which he explains all the variety of form and combination of the organs, as well as the terms expressing them. He establishes the rule of every rational nomenclature; and gives a general theory of the methods of distribution, and particularly of that which is called *natural*, because it is founded on the essential relations of vegetables to each other. On this subject he enters into various considerations, which are peculiarly his own, on the value of these relations, and on the organs and the conformations of organs on which they ought to be founded. He proposes new views on differences apparently very considerable between certain vegetables, and which merely depend upon the evertion or union of certain organs. Setting out from species in which this eversion or union is evident to the most careless observer, he proceeds with skill to those in which they may still be seen, but not so distinctly, and then he has only a single step to make to the eversions or unions indicated by analogy, though they can no longer be recognized by the sight, and the admission of which resembles those hypotheses which philosophers are obliged to make when they are destitute of facts, not to leave blanks in their developements. It is a method that may be dangerous in hands less skilful than those of M. de Candolle, but of which he has in general made an equally moderate and ingenious use. His work must be very useful, by introducing more and more a philosophical spirit into a part of natural history too much given up to routine, and which, notwithstanding all the progress which it has made, still contains among its cultivators a great number of servile imitators.

M. de la Peyrouse, Correspondent and Professor at Toulouse, has published an abridged history of the plants of the Pyrenees in one volume, 8vo. This work, which botany wanted, is principally owing to the numerous journies made by its author into that interesting chain. It comprehends a short description of all the plants which have been observed either by him or his predecessors, arranged according to the system of Linnæus, with the localities and references to the best figures which we possess of them. It constitutes an important acquisition to the French flora, and a useful guide to those who wish to visit these mountains.

It is not surprising that the history of sea animals is the department of natural history susceptible of the greatest additions. Living

in the bottom of the sea they escape observation, and even when they are caught they are seldom compared together ; so that a single species may have been seen by different observers, and have passed each time for new, when the first descriptions of it were not sufficiently complete, or when naturalists neglected to collect and study them.

M. Cuvier has presented to the Class some researches on fish thus forgotten, or multiplied in the catalogues of naturalists. One of these, remarkable for its great size, well known in Italy by the names of *umbra* and *fegaro*, and in Languedoc by that of *poisson royal*, was formerly well known at Paris by the name of *maigne.* It even gave origin to some popular proverbs. At present, from unknown causes, it has become rare in the English Channel, and therefore is scarcely brought to the capital. The naturalists of the sixteenth century have described it very well ; and Duhamel, in the eighteenth century, treated of it at great length; yet our systematic authors have either given it as new, or have confounded it with smaller and more common species. Besides an external description of it, M. Cuvier has given its anatomy, and particularly that of its swimming bladder, which is very curious, on account of the branched productions placed along its two sides.

Another species, which has been multiplied six-fold in the works of naturalists, and constituted into as many distinct species, is a small fish of the Mediterranean, which, from its red colour and general form, has been called *roi des rougets* or *rouget imberbe* (*mullus imberbis*, Linn. ; *apagon rouge*, Lacep.), but which has more relation to the perches than the *rougets.*

M. Noel de la Moriniere, who has been employed for some years on a treatise on useful fishes, has presented to the Class a memoir nearly of the same nature as the two preceding, in which he gives the history of a species much neglected by naturalists, though so numerous at certain seasons in the Gulf of Gascony, that the fishers of the Ile-Dieu alone catch every year more than 14,000 individuals, weighing each from 30 to 80 pounds. It is the *germon* or *grande oreille* of the French sailors, or *ala-longa* of the fishers of Sardinia (*scomber ala-longa*, Gm.),* so called because the principal character which distinguishes it from the tunny (*scomber thynnus*) consists in pectoral fins extremely long and pointed. Commerson having found at Madagascar a fish possessing the same characters, gave it the name of *germon*, and has been followed in this by Count Lacepede ; so that the *germon* of Europe is now particularly distinguished by the name of *ala-longa.* It remained to be known if the European and Madagascar germon be one and the same species. The distance of situation made the contrary probable ; and M. Geoffroi St. Hilaire has ascertained that this is the case by comparing the figure of the second left by Commerson

* Gmelin having written by mistake *ala-tunga*, this corruption has made its way into most subsequent books.

with the description of the first by M. Noël, and the figure of Pere
Plumier.   It would be proper, however, to confirm this result by
an actual comparison of the two fish.

M. Cuvier has likewise presented to the Class a fish little known,
lately caught in the Gulf of Genoa, four feet long, and shaped like
the blade of a cutlass, remarkable on account of an elevated crest,
surmounted by a kind of horn which it carries on its head, and by
very small ventral fins, placed below the pectoral.   We possessed
only an incomplete description of it by the late M. Giorna, a
naturalist of Turin,* who gave to the genus the name of *lophote*,
and had consecrated the species to Count Lacepede, as an homage
due to him from all those that study ichthyology.

M. Huber, of Geneva, son of the observer who has added so
many astonishing facts to the history of bees, already so surprising,
and author himself of a work on ants, filled with curious facts
respecting the instinct of these little animals, has presented to the
Class a memoir on the singular industry of a small caterpillar which
he calls *chenille à hamac* (hammock caterpillar), from the way in
which it suspends itself to pass its state of chrysalis.   It is of the
number of those that are called *mineuse*, and it lives in the leaves
of some fruit-trees.   In the month of August it ceases to eat, and
spins its hammock.   Five hours are sufficient to construct it.   Two
cords stretched between the edges of a leaf folded down and concave
above are its principal supports.   It is suspended there by ligaments
of silk, and two other ligaments fixed to the edges of the leaf keep
it, as it were, at anchor.   It has itself the form of a small cylin-
drical cocoon.   M. Huber, not satisfied with following attentively,
and describing with care, the successive operations of the little
architect who constructs this complicated structure, has endeavoured
to ascertain how far these operations are the consequence of reason-
ing in the caterpillar, and may be varied by her according to cir-
cumstances.   A caterpillar removed from the construction after it
has begun, begins it again as long as any silky matter remains to
her.   If placed upon a construction begun by another, she usually
continues it from the point at which she finds it.   But if the one to
which she is carried be far advanced, she prefers beginning the
whole anew.   The butterfly from this caterpillar appears to be the
phalœna clerkella of Linnæus, and one of its enemies is the
ichneumon ramicornis.

Our associate, M. de la Billardiere, has observed a curious fact
respecting the humble bee, or the large bee that makes its nest in
the earth under turf, stones, &c. He found at the end of autumn,
in the nest of the species called apis sylverum by Kirby, an old
female and a working bee whose wings had been cemented with
brown wax so as to prevent them from flying ; and he conceives it
was a precaution taken by the other bees to oblige these two indivi-

* Memoires de l'Academie de Turin for 1805—1808, p. 12, of the Memoirs.

duals to remain in the nest and take care of the larvas which next year were to renew the population of the colony.

M. Olivier, member of the Class, has drawn up a memoir on the insects injurious to corn, belonging equally to agriculture and zoology. Hitherto he has communicated only the part relative to the species which attack corn in the blade. M. Olivier describes nine such insects, all belonging to the order of insects with two wings. But he makes known at the same time three species of insects enemies to these, which, by stopping their propagation, diminish the injuries they commit.

One of the most important questions in the anatomy of insects respects the use of the great vessel which all that class have along the back, and which undergoes contractions and dilatations comparable to those of the heart and arteries. Malpighi and Swammerdam gave it the name of heart; but it appears certain, from the observations of Lyonnet and others, that no branches issue from it; and M. Cuvier seems to have established, by many proofs, that there is no circulation in insects. M. Marcel de Serres has examined this subject anew. He has satisfied himself, by very numerous observations on the largest insects in the south of France, assisted by all the most delicate instruments that anatomy possesses, that the dorsal vessel does not send off any ramification, that no other contractile vessel exists in the body, and no system of blood-vessels. Insects deprived of the dorsal vessel continue to live several hours, while the scorpions and spiders, which possess a true heart, perish immediately if it be destroyed. The contractions of the dorsal vessel are principally owing to the muscles of the back placed along its sides; but the tracheæ and nerves exercise a considerable influence on it. The humour which it contains frequently appeared of a colour analogous to that of the fatty matter, which always fills a part of the body; it is scarcely liquid, especially in the voracious larvæ. The diameter of the vessel is more equal in those larvæ in which the fat is more equally distributed, and the inequalities of its different parts are proportional to those of the fat in the corresponding parts of the body. The nerves and tracheæ are more abundant in the dorsal vessel of larvæ than in that of perfect insects; its contractions are also stronger, but less frequent. From these and some other facts the author considers himself entitled to conclude that the function of the dorsal vessel is to produce fatty matter, and that in order to produce this substance it absorbs a part of the nutritive liquor contained in the cavity of the body through the coats of the intestine, and that it then transudes it through the interweavings of the adipose tissue where the fat is completely formed.

M. de Serres has introduced into his memoir some valuable observations on the varieties of structure of the tracheæ in different families of insects, among which we may remark particularly those which relate to the mechanism of the vesicular tracheæ. He terminates

it by a statement of all the anatomical characters of the divisions
which he thinks it necessary to establish among articulated animals,
and especially insects. We regret that this laborious work, calcu-
lated to excite the most lively interest in the lovers of comparative
anatomy, is not of a nature to enter into our analysis. This con-
stitutes a fine addition to the observations of the same author on the
intestinal canal of insects, which we mentioned last year.

M. Montegre, a physician in Paris, has made a curious set of
observations on the habits of the common earth worm, and new
remarks upon the anatomy of these animals. They are herma-
phrodites, and each of them is productive ; and, according to the
author, produce small living animals They require copulation, but
without any intromission of parts, so that we may suppose it has for
object only to excite in them the movements necessary to produce
fecundation It takes place chiefly in the months of June and July.
The worms unite by means of a swelling which we observe at the
anterior part of their body, and which adheres closely to that of the
opposite individual. The young appear first in white organs placed
before, on the two sides of the stomach, and slide between the
intestines and the external muscles into a reservoir situated in the
thick part of the tail, where they are found full of life. The earth
worms exhibited no symptom to our observer to induce him to
suppose them affected by light or sound. But he has ascertained
that they do not live on earth, and he has found in their intestines
the remains of animals and plants.

We have spoken two years ago of the experiments of M. Leche-
nault on the deleterious effects of the juice known in Java by the
name of *upas*, when introduced into sores, as well as of those
of MM. Delille and Magendie, which tend to prove that this
poison acts essentially on the spinal marrow.

MM. Magendie and Delille, several times witnesses of the terrible
rapidity of its action, have been tempted to doubt whether it could
be carried so rapidly to the spinal marrow by means of the lymphatics,
and to examine whether we ought not, at least in certain cases, to
admit in the veins the absorbing faculty which was generally
ascribed to them before we were acquainted with all the branches
of the lymphatic system. To fix their ideas on this subject, they
applied upas to parts merely connected with the body by blood-
vessels ; for example, they cut off all the mesentery adhering to a
particular part of the intestine, leaving only the arteries and veins,
and after having put upas in the interior of that part, they cut it,
and tied its two ends. What appears still more conclusive, they
cut off a thigh, leaving only entire the artery and vein, and then
applied the poison to the foot. Finally, to remove even the notion
of invisible lymphatics belonging to the tissue of these blood-vessels,
they have removed a segment of each, putting in their place a tube
of quill, so that there was no longer any communication between
the limb and the animal but by the blood which circulated from the
one to the other. In all these cases convulsions and death took

place just as speedily as if the upas had been applied to the animal in a state of integrity. But perhaps some may object that when the upas was introduced into the intestine we may always suppose that some lymphatics remained concealed, and that when it was applied to the feet it was placed in a wound where it could penetrate into the blood by open veins, and that this is not what is meant when venous absorption is spoken of, for then we mean an action ascribed to the veins in their natural state, and by means of their organic pores. What is very remarkable in the experiments of MM. Magendie and Delille is, that the blood of an animal already poisoned, and just dying, transfused into the veins of another animal, does not kill it, and scarcely occasions any perceptible inconvenience.

M. Magendie has made another very interesting application of this action of certain substances introduced into the blood.

It is known that tartar emetic injected into the veins of an animal makes it vomit in a few minutes, while the same substance when swallowed requires an hour to produce the same effect. Hence it follows that the convulsive movement does not depend on the immediate action of tartar emetic on the coats of the stomach. Observations made on the viscus itself during the act of vomiting had led some physiologists farther. They had perceived that the walls of the stomach experience very little agitation, and hence had concluded that it is not in the irritation of these walls that the immediate cause of vomiting resides. But their opinion, which was feebly supported, had been almost forgotten since Lieutaud and Haller had rendered the contrary hypothesis prevalent.

M. Magendie, wishing to ascertain the truth, employed the convenient method of injections; and having at first made an opening into the abdomen, he ascertained by the touch that, during vomiting, the stomach itself remains in a state of inactivity; but that at each nausea it is violently compressed by the contraction of the diaphragm, and of the muscles of the belly. Farther, the long inspirations which precede each vomiting introduce a sufficient quantity of air to prevent its size from diminishing, notwithstanding the quantity of matter thrown out. If the abdomen be sufficiently opened to allow the stomach to come out altogether, the nauseas continue, but produce no effect, because the muscles that contract no longer act upon the stomach. If the stomach be replaced, vomiting begins immediately : yet compression alone is not sufficient; for if we compress with the hands the stomach of a dog thus displaced, to which no emetic has been administered, we expel indeed the contents of the stomach, but do not occasion true vomiting ; because there is neither nausea nor those inspirations which characterize that kind of convulsion. But if we stroke the stomach instead of compressing it, and if the strokes extend to the œsophagus, nausea and all the other symptoms of vomiting are produced without administering any emetic. Thus vomiting would appear to result from the compression of the stomach by a convul-

sive contraction of the muscles which surround the belly, and this contraction itself is excited by an irritation of the œsophagus.

It was of importance to know what muscles act principally, what nerves put them in action, and in consequence of what cause they are irritated. To determine these points, M. Magendie in the first place cut or removed the abdominal muscles without much diminishing the activity of vomiting. On the other hand, when a great part of its force is taken from the diaphragm by cutting the phrenic nerves, only weak nauseas take place at long intervals, and vomiting seldom takes place, notwithstanding the contraction of the abdominal muscles. Thus the diaphragm acts by far the greatest part in this compression. When the action of the diaphragm and abdominal muscles is destroyed at once, vomiting does not take place, even if the animal be made to swallow substances eminently emetic, as corrosive sublimate. Finally, what seems almost a wonderful corroboration of all these proofs, M. Magendie removed the stomach entirely, and substituted a bladder in its place, which was attached to the bottom of the œsophagus, by making it communicate with that pipe by means of a solid tube, and after sewing up the abdomen he injected tartar emetic into the veins. The animal had nauseas, made inspirations, and threw out a coloured liquid which had been put into the bladder; just as would have happened had the stomach been sound, and an emetic administered in the ordinary way.

Emetics, then, do not produce vomiting by irritating the fibres of the stomach, nor even the nerves; but in consequence of being carried by absorption and circulation to the nervous system, and exciting an action which reflects specifically on the œsophagus and diaphragm, so as to produce in them different movements, among which there are some the final result of which is the compression of the stomach. This does not hinder vomiting from being produced by the immediate irritation of the nerves of some one of these parts, or by any nervous irritation which affects the system in a way similar to an emetic.

It remains for M. Magendie to distinguish with more precision the part of the œsophagus and diaphragm concerned in the act of vomiting, and to examine the phenomena of this motion in birds and animals without a diaphragm.

To his memoir on the action of antimony considered in a physiological point of view, M. Magendie has added another on its medical or deleterious action; and he has shown by many observations made on man, and by numerous experiments on the inferior animals, that the tartrate of this metal taken in considerable quantity is of itself a mortal poison; but that its first effect is almost always vomiting, which throws out the greatest part of the poison before it has time to produce a fatal effect. Hence most of those who have attempted to destroy themselves by this substance have been disappointed in their object.

M. Magendie has likewise presented to the Class a set of experi-

ments relative to the use of the epiglottis. This, cartilage placed at the base of the tongue before the glottis in man and quadrupeds is generally regarded as destined by nature to hinder the substances swallowed from falling into the tracheæ. Birds and reptiles indeed have no epiglottis, and experience no inconvenience from the want of it; but their glottis is preserved by other means, such as the indentments with which it is usually surrounded, so that no objection against the received opinion can be drawn from them. Persons deprived of the epiglottis by accident, and yet capable of swallowing as well as ever, furnish a stronger objection; and some anatomists have even concluded that the epiglottis is rather subservient to the voice than to deglutition.

M. Magendie having removed the epiglottis of dogs, found that they could swallow as well as ever. He has ascertained likewise by actual inspection that the glottis contracts completely at the instant of deglutition, so that nothing would enter it, even supposing the epiglottis not to exist; and on cutting the nerves which go to the muscles that contract the glottis, he observed that it remained open, and admitted the aliments, notwithstanding the presence of the epiglottis which he had preserved.

It is difficult not to yield to experiments which agree so well with each other and with known facts. It is the business of physiologists to ascertain what may be the real use of the epiglottis, which is too large and too constant in a whole class of animals not to have some essential use.

M. Magendie has been led by his researches to examine the particular distribution of the laryngeal and recurrent nerves in different muscles of the larynx, and this part of his labour adds some precision to that part of anatomy.

The necessity in which Europe is at present of drawing all its resources from itself, and to produce in its territory all that has become essential to luxury and the arts, has induced different persons to endeavour to discover among the plants that may be cultivated in Europe species capable of furnishing the productions of the warm climates; and the principal efforts have been directed to the objects which are wanted in greatest abundance, because they promise the most immediate profit.

We have spoken already sufficiently of the sugar of beet-root, the preparation of which has been so much simplified as to render it likely that it will become for the Continent a durable object of industry.

M. Marsan, Professor at Padua, has read a memoir on another plant which yields sugar. It is more analogous to the sugar-cane in its botanical properties, and the sugar which it yields is equally good. It is a large grass, originally from the south of Africa, described for the first time in 1775 by Peter Arduino under the name of *holcus cafer*, and well characterized by its having glumes and its round seeds. It has begun to be cultivated in some parts of Italy, Bavaria, and Hungary.

It seems more difficult at present to find a substitute for coffee than for sugar. Attempts have been made to torrefy different kinds of grain and roots. But the liquids produced had nothing of coffee but the colour and bitterness.

M. Levrat, a physician at Châtillon on the Chalaronne, thinks that the seed of the common sedge (*iris pseudacorus*) is what approaches nearest to coffee. When dried, and freed from the husk that adheres to it, it is roasted and infused in the same way as coffee. He finds that the seed of the *iris* is a complete substitute for coffee in its febrifuge properties, and may therefore be used instead of *bark*. This would be a discovery the more important because as iris grows in marshes we could no longer reproach nature with not having placed the remedy near the disease.

Since bees are no longer destroyed in order to deprive them of their honey, various methods have been thought of to drive bees from one hive to another without being stung by them. M. Chambon, a physician at Paris, has proposed one which appears simple and sure. It is to have hives which may be opened above, to place them upon a metallic plate under which smoke is formed, and to put above the opening an empty hive, into which the smoke forces the bees to pass.

The same author has made experiments to determine whether it would be advantageous to clothe the sheep, as the ancients did, who boast greatly of this plan. He did not find that the wool increased sufficiently in beauty and value to pay the expense of the clothing.

M. Chambon has likewise read a memoir on the dangers which anatomists run in their dissections, and on the means of preventing and curing them. They are sometimes terrible, but luckily they are rare. The remedies and preservatives belong to those which physicians recommend against contagion and venomous sores.

M. Orfila, a young Spanish physician, has presented a great work on poisons considered with a view to medicine and jurisprudence. Hitherto the Class has only seen the first volume, which treats of the poisons from mercury, arsenic, antimony, and copper. The author has made many experiments on the difference which the presence of aliments occasions in the way in which poisons act when mixed with re-agents, differences which in certain cases may conceal their properties, and prevent them from being ascertained. He has pointed out all the precautions necessary to be taken by chemists to make a faithful report before a court of justice when they are consulted. He has examined with the greatest care all the methods proposed to stop the deleterious effects of these poisons, and to find new remedies when the old ones did not answer the purpose. The only antidote for corrosive sublimate, according to M. Orfila, is albumen or white of egg mixed with water; and that for verdigris is common sugar in pieces—fortunate discoveries to which theory would never have led.

M. Pictet, according to his plan of communicating to the Class all that his extensive correspondence brings to his knowledge relative to the sciences which we cultivate, has this year communicated

interesting observations on medicine and surgery. One of them would have certainly been considered as miraculous in those times when a pious credulity was pleased to see in each event the immediate intervention of a divinity—the cure of a man whose breast had been traversed by the pole of a carriage.* Another is of great importance, because it gives hopes of being able to cure hydrophobia—perhaps the most desperate disease to which mankind is subject. A man in a well marked hydrophobia was cured in India by bleeding, carried each time to deliquium, and repeated whenever there appeared a recurrence of the symptoms. The importance of such a discovery was so much the more felt because a few days before Baron Percy had read to the Class the frightful event which had taken place last October at Bar-sur-Ornain, where in a single morning a mad wolf bit more than 20 persons, all of whom died in consequence. A third observation, made at Geneva, and communicated by M. Pictet, was not so fortunate : a soldier having all the symptoms of croup underwent tracheotomy without success.

M. Pictet has likewise given us an interesting account of the plague which existed in the Russian port Odessa, drawn up by M. Charles Pictet, his nephew, who contributed successfully to stop its progress.

M. Portal has published an important work on the Nature and Treatment of Diseases of the Liver, in which he has communicated the result of his long experience on the disorders of an organ the great influence of which in health and disease is well expressed by the motto of the author—*Quanto magis ad sanitatem prodest, tanto et deterius in morbis afficitur.*

Our respectable associate M. Tenon, who, notwithstanding a delicate temperament, and a youth which did not promise him long life, has preserved by care the faculties of his body and mind so long beyond the time allotted to the generality of mankind, has chosen to leave us the secrets which he has so successfully practised on himself. His Offering to Old People of some Methods of Prolonging their Lives, is a code of longevity dictated by science and experience ; but to draw the same good effects from it as the author, we must, like him, join to it a tranquil situation, agreeable occupations, and the calm of a beneficent and pure mind.

ARTICLE XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Mr. Singer will commence a Course of Lectures on Electricity and Electro-Chemistry at the Russell Institution early in January,

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* This alludes to the well-known case of Mr. Tipple, which took place two ears ago in the neighbourhood of London.—T.

Nearly the whole of his very powerful apparatus, which has been recently materially improved and extended, will be employed in illustration of these lectures.

I. *Mean Temperature of different Places.*

THE following table, drawn up by M. de Humboldt, is well worth the attention of meteorologists. Those cities to which an asterisk is attached are singularly situated with respect to climate, either by their elevation above the level of the ocean, or by circumstances independent of the latitude :—

Places.	Latitude.		Mean Annual Temp.	Observations.
Umeo................	63°	50′	33·25°	(Naezen and Von Buch.)
Petersburgh	59	56	38·84	(Euler.) Very eastern position.
Upsala	59	51	41·90	(Von Buch.)
Stockholm............	59	20	42·26	(Wargentin.)
Copenhagen	55	41	45·68	(Bugge.)
Berlin................	52	31	46·58	
Paris	48	50	51·26	(Bouvard.) Average of 7 years.
Geneva *	46	12	50·18	Height 819·77 English feet.
Marseilles	43	17	57·74	St. Jacques.
Toulon *	43	8	63·50	Mountains to the north.
Rome	41	53	60·26	(William de Humboldt.)
Naples	40	50	64·40	
Madrid *	40	25	59·00	Height 1248 English feet.
Mexico *	19	25	62·60	Height 4713½ English feet.
Vera Cruz *..........	19	11	77·72	Dry sandy coast.
Equator at the level of the Ocean	0	0	80·60	
Quito	0	14	59·00	Height 6020 English feet.

If the reader compare this table with one given by Dr. Wahlenberg, and published in the *Annals of Philosophy*, vol. iv. p. 115, he will find some slight differences, though not material. It would be easy to add to the preceding table the mean temperature of different places in Great Britain ; and from our insular situation this temperature is much higher than might be expected from the latitude of the places. Thus the mean temperature of Edinburgh is 47°, judging from the springs, which I conceive to be rather below than above the truth. The latitude of Edinburgh is 55° 58′ 57″; yet we see its mean temperature not only exceeds that of Copenhagen, but even that of Berlin, which is nearly 3¼° farther south. In like manner, the mean temperature of London determined from the heat of the springs is 51°. Now this is only one-fourth of a degree of the thermometer lower than the mean temperature of Paris, though the difference of latitude be more than 2¼°. These examples are sufficient to show how much the winter cold in Great Britain is moderated by the insular situation ; for the difference in our favour falls chiefly in winter. In summer, continental places are almost always much warmer than insular situations in the same latitude.

II. *Interior of Spain.*

The middle parts of Spain, consisting of the two Castiles, and several contiguous provinces, constitute a high tabular land, the climate of which is much colder than might be expected from the latitude. We have seen in the preceding table that the mean temperature of Madrid is only 59°, though it is situated farther to the south than Naples, where the mean temperature is 64·4°. According to M. Bauza (as quoted by Humboldt), the mean height of the barometer at Madrid is 27·923 English inches. This, according to Humboldt's calculation, gives a height of 1248 English feet above the level of the sea. If we consider this as the mean height of the interior tabular land of Spain, we need not be surprised at the difference between the mean temperature of that country, and of the south of Spain, where the country is low, or of Naples, which is in the same predicament. It seems in general to hold, at least in this country, that for every 280 feet of elevation the thermometer sinks one degree. According to this law, the mean temperature of Madrid ought to be 4·4° lower than that of Naples; but we see from the preceding table that it is 5·4° lower.

III. *Highest Point of Spain.*

According to Humboldt, the highest mountain in Spain is not Mount Perdu, in the Pyrenees, as has been hitherto supposed, but the Mulahacen, which forms part of the Sierra Nevada of Grenada. This peak, according to the measurement of Don Clemente Roxas, is 5984·4 English feet above the level of the sea, while Mount Perdu is only 5784·2 English feet above the same level.

IV. *Height of Mountains.*

It is well known that the highest mountains in Europe are the Alps, and that Mont Blanc, the highest of the Alps, is 15,603 English feet above the level of the sea. The Pyrenees seem to come in the next place after the Alps. The highest summit, known by the name of Pic Blanc, is elevated rather more than 10,000 English feet. Next to the Pyrenees, the highest chain in Europe is that which separates Norway from Sweden, known in this country by the name of the Doffrine Hills. The following are the heights of the highest of these mountains, according to the most accurate recent measurements :—

Syltoppen	6652 Eng. feet
Sulitelma	6178
Olmajolos	5543
Oreskutan	5308

But several mountains occur in Europe higher than any of the Doffrines. Thus Mount Ætna exceeds 10,000 feet. None of the mountains in Germany seem to rise to a great height. Schneekopf, the highest summit of the Riesengebirge, is stated at 4950 feet. I

presume they are French feet, though I am not certain. The highest mountain in Spain, according to Humboldt, is only 5984 English feet in height ; and he sinks Mount Perdu, hitherto considered as the highest summit of the Pyrenees, to 5784 feet, which is only about half the height of the old measurements. Ben Nives, the highest mountain in Great Britain, is only 4350 feet above the level of the sea.

The following table exhibits the heights of some of the most remarkable mountains in America, or the neighbouring islands:—

Mowna Roa	16614 Eng. feet
Chimborazo	22700
Pinchinca	16545
Cotopaxi	20325
Pic d'Orizaba	17368
Popocatapetl	17716

V. *Baron de Humboldt.*

It is understood that this celebrated and enterprising traveller is shortly to set out for Asia to explore the hitherto almost unknown country of Thibet. No object of greater importance can well occupy either a geologist, botanist, or zoologist, or one who is attached to the improvement of geography. This country seems beyond a doubt to be the highest in existence ; some of its mountains, if any reliance can be put in geometrical measurements made at a great distance, and consequently liable to the greatest possible errors from refraction, arise to the enormous height of 27,000 English feet above the level of the sea. How interesting will it be to learn the nature and structure of these enormous mountains, and the vegetable and animal remains which crown their summits.

VI. *Tantalum.*

I have received the following notice from Dr. Berzelius, in consequence of a change which I made in the nomenclature employed by him in his Essay on the Cause of Chemical Proportions. I substituted *columbium* for *tantalum*, and assigned as a reason that I thought the first discoverer had always a right to impose a name upon what he discovered. To this Berzelius has made the following answer :—

" You have changed the name tantalum into columbium, because in your opinion the discoverer of it was Mr. Hatchett, and not Mr. Ekeberg, and because you consider the name columbium to be just as good as tantalum. Without attempting to depreciate the merit of the celebrated Hatchett, we must do justice to the late Mr. Ekeberg. Now I affirm that neither the properties of tantalum nor of its oxide were known before the experiments of Ekeberg. Mr. Hatchett described the oxide of columbium as an acid but little soluble in water, which reddened infusion of litmus. It is soluble

both in muriatic and sulphuric acid, when concentrated and boiling. It combines readily with the alkalies, both caustic and carbonated, when boiled in an alkaline ley. With microcosmic salt before the blow-pipe, it melts into a blue glass, with a shade of purple.

" Mr. Ekeberg describes the oxide of tantalum as follows:— ' This oxide may be reduced into an infusible metallic substance, having the grey colour of iron. The oxide is insoluble in acids, particularly the nitric and muriatic. If an alkaline solution of tantalum be poured into a greater excess of concentrated muriatic acid, a small quantity of the oxide remains in solution in the acid, but the greatest part is precipitated. To combine this oxide with an alkali, it must be fused with several times its weight of fixed alkali. Carbonic acid precipitates it from its alkaline solutions. Fused with microcosmic salt, it gives a transparent and colourless glass.'

" Ekeberg had received from a friend, who had visited England, a small portion of Mr. Hatchett's columbic acid ; and when he was informed of the result of Dr. Wollaston's experiments, he made a careful examination of that acid. He found that it contained a considerable proportion of tungstic acid. To this it owed its acid properties, the ease with which it combined with alkalies, and the blue colour which it communicated to microcosmic salt. He ascertained that the other constituent of columbic acid possessed the properties of oxide of tantalum. The substance then called columbic acid was a mixture of tungstic acid and oxide of tantalum.

" These observations of Mr. Ekeberg have gained still more weight by the discovery of a new mineral by Mr. Gahn and myself, in the environs of Fahlun. It has a very close resemblance to the columbite of Mr. Hatchett, and we have found that it consists of tungstic acid combined with oxide of tantalum, oxide of iron, and oxide of manganese. We shall publish hereafter our experiments on this mineral and upon tantalum, which will be found conformable to the opinions of Ekeberg. From this statement, it is clear that Hatchett participates with Ekeberg the discovery of tantalum, in the same way as Fourcroy and Vauquelin participate with Tennant the honour of the discovery of osmium. (See Thomson's Chemistry, 4th Edit. vol. i. p. 200.) I suppose you will not refuse the same justice to the Swede Ekeberg that you have done to the Englishman Tennant.

" As to the name of the metal, I do not think that the claim of the original discoverer is great. You do not, for example, say menachan in place of titanium. Mr. Hatchett chose the name columbium from the place from which he supposed the specimen came ; but this is not a good method of naming minerals. Besides, in the present case the locality is doubtful. We are not sure that the specimen came from America. The word tantalum has none of these inconveniences ; and as it points out, besides, some of the most striking properties of the mineral, I think that it ought to be employed in preference. Perhaps the name tantalum (from the

history of Tantalus) will appear still more appropriate, when I add that the metal, when in the state of the finest powder, is not attacked by any acid, not even by the nitro-muriatic."

I have only to observe, in answer to all this, that I have never had an opportunity of making a single experiment on columbite or tantalite, nor of perusing Mr. Ekeberg's experiments. In the last edition of my System of Chemistry, the reader will find that I omitted tantalum altogether, because I was not in possession of accurate information on the subject. Supposing the preceding statement correct, we cannot hesitate to admit Ekeberg to be the discoverer of this metal; but I own, the circumstance of neither Mr. Hatchett nor Dr. Wollaston having observed the presence of tungstic acid, throws some doubts on Ekeberg's statement. It is possible that this may have been owing to the small quantity of columbite examined by them. I trust Mr. Hatchett will still have it in his power, by a new examination, to verify or refute the statement of Ekeberg. Dr. Berzelius' views respecting nomenclature differ so much from mine that we should seldom agree about terms. If a name given to a chemical substance had no previous meaning, I think it so much the better. I believe that *oxygen, hydrogen, platina*, and a multitude of others, have now lost entirely their original meaning, and are used solely as the names of peculiar substances. In like manner, I think *strontian* just as good a name as *barytes*, and *hornblende* far better than *amphibole*. I conceive too that Christopher Columbus has as good a claim to give his name to a metal as King Tantalus.

VII. Dr. Olinthus Gregory.

Ecce iterum Crispinus.—Juv.

In the last number of this Journal I mentioned this Gentleman's attack upon the Editor of the *Annals of Philosophy*, explained the nature of the dispute (if dispute it can be called, when there is only one disputant), and the extraordinary lengths Dr. O. Gregory had gone in order to wreak his vengeance on the Royal Society. There still remain a few particulars which in justice to myself ought I think to be stated.

1. It has been observed as a general law that the length of a degree gradually increases as we advance from the equator to the pole. The measurements in Peru, over all France, and at Torneo, all show this. The Newtonian theory presupposed and requires this law. But the measurement of a few degrees in the south of England gives just the opposite result. They increase in length as we proceed from the north to the south. When such an anomaly occurs, we ought to be very sure of the accuracy of the measurement before we admit it. Every probable supposition ought to be tried in the first place. Don Rodriguez has shown that if an error of $4\frac{1}{8}''$ had been committed at Arbury Hill, the anomaly would

disappear. Here is one supposition certainly entitled to examination. Nor do I think that the trouble and expense of repeating the observations at Arbury Hill would be great. No, says Dr. Gregory, the instruments were so good that such an error was not possible. Dr. Maskelyne, who understood observing fully as well as Dr. O. Gregory, used to say that he did not think that the best instruments would warrant a nearer approximation to the truth than 5″ ; so that what Dr. Maskelyne considered as the most accurate possible of observations, Dr. Gregory thinks so inaccurate that it is impossible to observe so ill. Yet he allows much greater errors to have been committed by the French, in whose measurement, however, no such anomaly appeared. So far from it being criminal in the Royal Society to publish Don Rodriguez' paper, it was their duty to publish it, in order to draw the attention of philosophers to the removal of such an anomaly, if it be possible.

2. Dr. Olinthus Gregory informs us that he wrote to M. Delambre on the subject of Don Rodriguez' paper, and that the French astronomer had given it as his opinion that Don Rodriguez' notion was untenable. Now it happens that M. Delambre has lately published a System of Astronomy, in which he has given us his opinion of Don Rodriguez' paper. I beg leave to lay the words of this respectable writer before my readers :—" Posteriurement a la mesure de Laponie, le Colonel Mudge mesura trois degres en Engleterre, entre Clifton et Dunnose dans l'ile de Wight. Par la station d'Arbury il a partagé son arc en deux parties presque egales. Il a soudivisé ses parties en liant ses triangles à l'observatoire de Greenwich et à celui du Lord Marlborough. On a été fort surpris que tous les arcs partiels comparés entre eux, indiquassent un alongement. Mais en laissant de coté Greenwich et Blenheim, M. Rodriguez a montré dans les Transanctions Philosophiques de 1812, que l'arc entier s'accorde avec le demi-grand axe et l'aplatissement qui resultent de nos mesures ; qu'il suffirait ensuite de supposer une erreur de 5″ a la station intermediaire d'Arbury, pour que les deux arcs partiels appartinsent au meme spheroide. Il restera pourtant a concevoir comment le beau secteur de Ramsden dont l'erreur etait constant ou nulle aux extremities, a pu avoir un erreur different de 5″ a la station intermediaire. Mais que ces anomalies proviennent de l'instrument au des inegalités locales, ce qui parait n'être pas encore bien decidé, il en resultera toujours que dans les operations de ce genre il n'y a de sureté que dans les arcs d'une grande etendre, tel que celui qui va de Dunkerque à Barcelone ou Formentera, ou l'erreur, quellqu'en puisse etre la cause, se trouve divisé par un plus grand intervalle. Dans les arcs trop petits et contigus, l'erreur inevitable des observations doit avoir un trop grande influence sur le resultat, qui ne peut repandre aucun jour sur la question de l'aplatissement."*

* Astronomie Theorique et Pratique. Par M. Delambre, 1814, vol. iii. p. 325.

I leave Dr. Olinthus Gregory to reconcile this passage as well as he can with his own statement of M. Delambre's opinion. I shall merely observe that some attention to truth, when a man enters into a dispute, is rather for his ultimate interest than the contrary conduct. I would therefore recommend to Dr. O. Gregory in his next dispute to try the effect of this line of conduct by way of experiment: it would contribute greatly to shorten the dispute, and would save a great deal of time and paper.

3. I observed on a former occasion that mathematicians were very apt to indulge in trifling or absurd speculations. This Dr. O. G. calls stigmatising the whole class. As he did not understand my meaning, I shall explain myself more fully. Mathematicians in general are careless about the data from which they reason, and think that they have no concern with any thing else than the accuracy of the calculations themselves. This is particularly the vice of French mathematicians; but those of no country are entirely free from it. I know only one modern mathematician who never reasoned except from the most careful and accurate data. He stands a striking exception. I mean Sir Isaac Newton; and we all know how high he stands, not merely as a mathematician, but likewise as a philosopher. When a mathematician reasons from false or absurd data, he may show us the resources of his art, and that he is master of calculation; but his labour has no tendency to advance our knowledge of nature. Therefore I disapprove of such conduct. If Dr. G. will read any mathematical treatise on hydraulics, or look into the works of some of the most celebrated modern French mathematicians, he will find plenty of examples of the fault to which I allude.

4. As to Dr. O. Gregory's triumphant statement, that the Royal Society were bound to print the whole of the *Survey* because they printed the preliminary particulars, which were interesting to men of science in general, I leave him in full possession of it. Valeat, quantum valere potest.

The preceding statement appeared to me necessary: I have therefore given it. But the readers of the *Annals of Philosophy* need not be afraid of being any longer fatigued with this subject, which I now drop, unless farther falsehoods on the part of Dr. O. Gregory should make it necessary to state a fact or two which I still keep in reserve.

VIII. *Live Shell-Fish in the Moss near Elgin.*

(To Dr. Thomson.)

SIR,

I take the liberty of requesting information, through the medium of your *Annals*, as to the particulars concerning some live shell-fish, which are said to be found at the depth of three or four feet in the solid body of a moss near Elgin, in Murrayshire. By doing this, Sir, you will greatly oblige, your most obedient,   C. T.

Nov. 8, 1814.

IX. *Compact Felspar.*

A specimen of supposed compact felspar, from Siebenlehn, analyzed by Klaproth, was composed of

Silica	51·0
Alumina	30·5
Lime	11·25
Soda	4·00
Oxide of iron	1·75
Water	1·25
Loss	·25
	100·00

X. *Topaz.*

Great numbers of topazes have been lately brought to this country from Botany Bay. Mr. Mawe showed me parcels of them, which had exactly the characters of the topaz of Scotland. As this last is much scarcer and dearer than the Botany Bay topaz, there is reason to fear that the latter may be substituted for the former.

ARTICLE XIII.

Scientific Books in hand, or in the Press.

Mr. Thomas Forster will publish in a few days an edition of the Diosemea of Aratus, with the Scholia, Notes, and a collation of parallel passages, so as to comprehend the whole of the Ancient Doctrine of Prognostics. The greatest part of the Meteorological Excursor, which were to have followed at the end, having been consumed by fire, the author can only insert a fragment of them, being the labour of two years.

Mr. Bakewell is preparing for the Press a Second Edition of his Introduction to Geology, which will be considerably enlarged, particularly by information acquired during the author's recent examination of different parts of England, and the Eastern Coast of Ireland. This edition will contain a notice of the most important geological discoveries and observations which have been made on the Continent of Europe, and in various parts of the globe, since the publication of the former, and is intended to comprise a view of the progress and present state of the science.

A New Edition, considerably enlarged and improved, of Dr. Lettsom's Naturalist's and Traveller's Companion, will be published next month.

Lieut. Gen. G. Cockburn's Narrative of his Voyage up the Mediterranean in 1810 and 1811 will be shortly published. It contains an account of a tour in Sicily, Malta, and the Lipari Islands.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1814.	Wind.	BAROMETER. Max.	Min.	Med.	THERMOMETER. Max.	Min.	Med.	Evap.	Rain.	
10th Mo.										
Oct. 6	S W	29·73	29·63	29·680	59	39	49·0	·10	—	☽
7	N W	29·83	29·73	29·780	57	32	44·5	—	—	
8	N	30·01	29·83	29·920	55	31	43·0		—	
9	N W	30·16	30·01	30·085	52	24	38·0		—	
10	N	30·16	29·91	30·035	56	34	45·0		—	
11	S	29·91	29·68	29·795	54	41	47·5		—	
12	S W	29·68	29·57	29·625	59	44	51·5	—	—	
13	S E	29·57	29·40	29·485	64	50	57·0	1	·32	●
14	S W	29·49	29·31	29·400	67	48	57·5	·13	—	
15	S W	29·48	29·40	29·440	59	42	50·5	·10	—	
16	S W	29·68	29·48	29·580	56	38	47·0	—	—	
17	S	29·68	29·28	29·480	58	43	50·5	—	—	
18	S E	29·40	29·13	29·265	58	36	47·0	·14	—	
19	S W	29·25	29·03	29·140	54	42	48·0	·80	—	
20	W	29·68	29·25	29·465	55	30	42·5	—	—	
21	W	29·73	29·68	29·705	55	33	44·0		·30	☾
22	S W	29·55	29·50	29·525	59	47	53·0	4	—	
23	N W	29·74	29·54	29·640	57	34	45·5		—	
24	S	29·74	29·09	29·415	53	39	46·0	·95	—	
25	N W	29·50	29·09	29·295	52	32	42·0	6	—	
26	N E	29·70	29·50	29·600	50	38	44·0	—	—	
27	W	29·81	29·70	29·755	53	32	42·5		—	
28	N E	29·82	29·75	29·785	52	32	42·0		·17	○
29	S E	29·84	29·66	29·750	52	37	44·5	·13	—	
30	Var.	29·90	29·87	29·885	53	39	46·0		—	
31	N E	29·93	29·87	29·900	55	47	51·0		—	
11th Mo.										
Nov. 1	N E	29·96	29·93	29·945	50	43	46·5	4	—	
2	N E	29·96	29·93	29·945	44	38	41·0		—	
3	N E	29·93	29·83	29·880	47	33	40·0		·10	
		30·16	29·03	29·662	67	24	46·43	2·50	·89	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Tenth Month.—6. Misty morning : the trees dripping : the wind veered from N. W. to S.W. with *Cirrostratus*, then *Cumulostratus*, and finally rain. 7. Clear, a. m.: much dew, p. m. : a sudden shower, with hail. 8. Hoar frost: fine day, with *Cumulostratus.* 9. Hoar frost : *Cumulus :* fine day : the wind a strong breeze : clear orange sky in the twilight. 10. Hoar frost : clear. The temp. of last night was destructive to various tender garden plants, and brought down the leaves from the mulberry trees. 11. a. m. Wind S. : the sky turbid, and streaked from N. to S. 12. Overcast, with *Cirrostratus :* it appears to have rained in the night. 13. a. m. large *nimbiform Cirri*, passing to *Cirrocumuli :* the wind E., very gentle, the vane remaining at S. W. : a smart breeze from this quarter ensued. 14. The lighter modifications of cloud, with much wind, a. m. : driving showers, p. m. 15. Fine, a. m. : then wind, and some showers. 16. Clear : windy. 17. The sky veiled with *Cirrostratus.* 18. Much wind : small rain at intervals : then showers. 19. Windy : wet, a. m. : clear at noon : p. m. rain again, by inosculation of the clouds : rainbow : wet, stormy evening. 20. a. m. rather windy, with *Cirrostratus* and *Cumulus :* large *Cirri* and *Cirrocumuli* above the wind : a few drops of rain. 21. Hoar frost : calm. 22. Overcast : wind, followed by rain in the night. 23. p. m. *Nimbi* to N. and S. : a rainbow in the former. 24. A fine day : *Cumulus*, with *Cumulostratus*, and extensive beds of the lighter clouds above, which appeared to descend, passing to the westward : the evening was overcast, and there followed a tempestuous night : the wind was mostly S. or S. E., changing in the morning to N. W. The barometer had been lower than the minimum here noted. 25. Showers, a. m. : cloudy, p. m. : clear night. 26. Hoar frost : somewhat misty : a little rain followed. 27. A fine day, after a cloudy morning : large distinct *Cumulostrati*, in an air nearly calm. 28. Misty morning : minimum temp. about seven : the drops of dew frozen : a fine day. 29. a. m. *Cirrostratus*, overcast sky : p. m. showery, with wind. 30. The lighter modifications appeared till noon, in elevated lines stretching N. E. and S.W., the wind being N.W.

Eleventh Month.—1. Cloudy : a steady breeze from N. E. and small rain. 2. The breeze continues. 3. The same : overcast morning : fine day.

RESULTS.

Wind for the most part Westerly : but during the latter part of the period Easterly.

Barometer : Greatest height......................30·16 inches ;
　　　　　　　Least..............................29·03 inches ;
　　　　　　　Mean of the period ....29·662 inches.

Thermometer : Greatest height ....67°
　　　　　　　　Least.................................24°
　　　　　　　　Mean of the period ....46·43°

Rain, 2·50 inches.　　Evaporation, 0·89 inch.

Tottenham, *Eleventh Month*, 11, 1814.　　　　　L. HOWARD.

INDEX.

END OF VOL. IV.

ERRATA IN VOL. III.

Page 99, line 21, *for* crucible, *read* retort.
—— 104, —— 27, — dried in a red heat, *read* strongly dried.
—— 105, —— 37, — $\frac{1}{2}$ or $\frac{1}{4}$, *read* $\frac{1}{2}$ or $\frac{1}{4}$ per cent.
—— 248, —— 23, — 57·3, *read* 37·3.
—— 256, —— 44, — amber, *read* umber.
—— 257, —— 26, — peroxide, *read* permuriate.
—— 355, —— 32, — Rolhoff, *read* Rothoff.
—— 356, —— 29, — O, *read* 2 O.
—— 357, —— 18, — 693 64, *read* 684·74.
—— 357, —— 7, from bottom, *for* Swedish minerals, *read* Swedish iron ores.
—— 361, —— 20, *for* each substance, *read* each compound substance.
—— 361, —— 40, — weight, *read* specific weight.
—— 437, —— 33, — spirts piss, *read* spits pus.

ERRATA IN VOL. IV.

Page 257, 8 lines from bottom, *for* a center of itself, *read* a center out of itself.
—— 258, line 27, *for* Gideon, *read* Gibeon.
—— 335, —— 2, last column of the table, *for* 2·80, *read* 3·80.
—— 336, —— 24, *for* on, *read* or.
—— 336, —— 32, — on, *read* through.
—— 397, —— 29, — investigations, *read* inventions.